# THE JOURNAL OF PARAPSYCHOLOGY

Volume 15 SEPTEMBER, 1951 Number 3

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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

DUKE UNIVERSITY PRESS
DURHAM, N. C.

# *The* Journal *of* Parapsychology

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This JOURNAL is published on the fifteenth day of March, June, September, and December by Duke University Press, Durham, North Carolina.

Contributions submitted for publication and all editorial communications should be addressed to the Managing Editor, Dorothy H. Pope, College Station, Durham, North Carolina. Correspondence with the editors is advised before submitting articles other than reports of experimentation. Since it does not forward manuscripts by registered mail, the JOURNAL cannot guarantee that they will not be lost in transit, and contributors are urged to keep copies of their papers. All contributions should be typewritten, double spaced. References should be given in the form adopted by the JOURNAL.

Reprints may be ordered when the proof is returned. The JOURNAL will bear one half the cost of reprints up to two hundred copies. Correspondence concerning subscriptions, change of address, back numbers, and other business communications should be addressed to Dorothy H. Pope, College Station, Durham, N. C.

The subscription price is $6.00 a year; single current numbers $1.50. The rate for back volumes is $7.00; for single numbers $1.75. Missing numbers will be supplied free when lost in the mails if written notice is given within one month of the date of issue. All remittances should be made payable to the JOURNAL OF PARAPSYCHOLOGY.

Subscriptions may be obtained in Great Britain through the Cambridge University Press, Bentley House, Euston Road, London, N. W. 1.

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Entered as second class matter May 21, 1942, at the post office at Durham, N. C., under the act of March 3, 1879.

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DUKE UNIVERSITY PRESS

*The* Journal *of*
# Parapsychology

Volume 15 SEPTEMBER, 1951 Number 3

## EDITORIAL

## THE OUTLOOK IN PARAPSYCHOLOGY

THERE IS ALWAYS in every field a certain interest in viewing things in prospect. Such a picture is important, too, because judgments of the future, however tentative, may considerably influence present action and current evaluations. In recent numbers of the JOURNAL we have already given editorial surveys of certain aspects of the current outlook: the shifting scene in methods, emphases, and bearings; an inventory of findings; and various appraisals of the relation of parapsychology to adjacent branches of inquiry. It will be in keeping with what has already preceded to attempt now to look ahead a little, not in an effort at specific prediction but rather at picturing the trends that are now discernible.

The most distinctive trend, perhaps, is the development in parapsychology of signs of maturity. This is exemplified in the very way the more active experimental workers are able to coordinate their efforts, meet each other on common ground, and in general concur as to acceptable standards, methods, and problem areas. Much of this collaboration, it is true, is expressed in personal exchanges of correspondence and conferences and only a small part of it reaches the stage of joint publication. This small part, however, is well represented in the present issue of the JOURNAL by the Soal and Pratt paper. And those who follow the news notes on parapsychology know that there have been in recent years a

number of important exchanges and visits both ways across the Atlantic in what has been a mutually helpful meeting of minds and sharing of professional contributions. These marks of more mature interrelationships appear to be increasing in number to the extent that they indicate a trend.

Also, in spite of the fact that there is as yet no long-term financial security anywhere in parapsychology, a growing number of full-time research workers are venturing into the field, and others are waiting for the opportunity to do so. We can safely foresee a steady growth in the number of professional personnel through the years ahead. The increased participation, too, of experts from other fields whose services are needed promises much for the strengthening of the research organization. More mathematical and statistical assistance than ever is available. Able physicists, psychiatrists, psychologists, and biologists—to name only a few of the professions represented—have indicated in greater numbers than ever before an interest and a readiness to deal with problems raised by parapsychology. The prospect that the hitherto isolated psi researches will now be more firmly buttressed with collateral aid is a reassuring one.

There is the beginning of a still broader spread of parapsychology's interrelations. Already in adjacent fields certain uses of the psi findings are being made or attempted. This is only an intellectual use, as yet, but such utilization is nonetheless important. Here and there a psychologist or two have integrated psi into their schemes of personality, a scattering of psychiatrists have worked ESP into their theories and (in an exploratory way) even into their practices; one biologist sees ESP as a probable factor in evolution, another appeals to it to help to explain homing behavior; a neurophysiologist brings it into place in his theory of thought-brain relations; a sociologist or two recognize it as a part of the answer to the cultural blight of materialism; one philosopher uses psi in defense of Direct Realism while another suggests that clairvoyance may be the basis even of sensory perception; and a few religious leaders cautiously accept it as confirming a conception of man essential to religion. These intellectual applications of the psi research findings constitute the kind of recognition that counts most for a

beginning science. This rational utilization is no mere empty academic acceptance of the discoveries made in psi, but a readiness to put them to work on the level of ideas—the only level on which they are as yet applicable.

There is, on the other hand, one general prospect for parapsychology that many readers will not unreservedly welcome. It is the inevitable trend toward technicality. Healthy though this sign of successful growth may be, it will alienate some of those who wish to follow every step of progress with full understanding, but who find increasingly greater demands made upon their capacity to understand the more involved terminology, the more complex mathematics, and the growing system of concepts and generalizations. We can safely predict, nevertheless, that there will be no time in the future when parapsychology will be simpler than it is today, or even so simple, for that matter. We can only hope that there will be an attendant development of competent interpretative writers whose skills will be brought to the important task of making scientific findings understandable. Along with this prospect of increased technicality, however, there is the compensating assurance of a more integrated science of parapsychology. The field has already become more than the odd assortment of unclassifiable problems that constituted psychical research twenty-five to fifty years ago. To a degree the advance in the organization of findings counterbalances the growing complexity.

* * * * *

But more specifically, what will be the main emphases in the research of the period just ahead? Surely the continued return to the use of spontaneous cases for fresh orientation in the research will constitute one of the trends. The first example of this approach is to be found in Louisa E. Rhine's article in this number. Her report illustrates something of the potential that lies in this type of collection, once safe modes of treatment and interpretation are adopted.

One incidental aspect of this new method of case study is the opportunity it offers for the participation of a large number of co-workers who wish to assist in the general research program but who, for one reason or another, are not situated so as to be

able to join a laboratory staff as full-time workers. The collecting of the cases itself offers opportunity for a wide range of collaborators, and there are other aspects, in the study and analysis of the materials, that present special opportunities for the investigator.

Turning to the general heading of the psychology of psi, we shall select three general fronts from which the prospect may be viewed, all three related one to another, yet each a topic in itself. On the first of these, we may surely count upon a broader foundation for psi in the psychology of personality if the leadership of Drs. Schmeidler and Humphrey alone continues, and so promising a field will surely appeal to others as well. Parapsychology may be said now to have passed the point of establishing the fact that personality differences are important in performance in psi tests. No longer is there special point merely to showing that high and low scorers can be differentiated significantly by the use of quantitative estimates of personality traits or states. This achievement has been repeated often enough that now more advanced goals will be set; indeed, they already have been.

Minds are now at work attempting to see pattern and reason to the personality correlates that have thus far been isolated. Clearer focus is developing, and already caught in the sharpened focus is the fact that *it is not the psi process itself that is represented in the correlations of psi scores with the principal personality measures but rather another personality device,* though one closely involved in the psi test response. It is one, however, that stands squarely across the doorway of psi operation and determines whether psi will register on the side consciously intended (that is, as a positive response) or on the other side (as a negative one), a rejection of the ostensible target or goal. This plus or minus attitude (or "sign factor") is, of course, connected with a wide range of unconscious mental activities. The understanding and control of it will be a major achievement.

One by one, it may be anticipated, such surface factors as this sign function may be expected to be isolated and effectively removed so as to allow the deeper-lying operation of psi to be brought into closer scrutiny. In addition to this sign factor is another one that causes much difficulty in getting at the ESP process itself, the factor responsible for the declines in scoring rate that seem almost inevi-

table if a subject continues through a series of many trials in a psi experiment. The decline factor or factors, too, can be completely inhibitory to psi, and if the real port of entry into ESP is ever to be cleared, the decline factor, like the sign function, will need to be brought under penetrating observation. Efforts at the study of personality states with relation to psi that do not take these special doorkeeper functions into account will almost certainly be doomed to failure.

* * * * *

The second psychological front on which we may expect progress if we follow up our leads to good advantage is concerned with the part the target and its conditions play in psi. Ever since A. A. Foster's paper in 1938 on the "diametric" nature of ESP in blind-matching tests, the pattern-character of extrasensory perception has been known. Foster pointed out that the subject's response in a blind-matching ESP card test suggests that he reacts to a relationship, a pattern or whole (without a step-by-step identification of each of the two cards first). Much of the analytic work done in recent years on the distribution of success on the record sheet also indicates a configurational type of perception—perception of a relation involving more than one element, sometimes several.

An excellent example of the patterned character of ESP is provided in the "reinforcement" effect reported by Soal and further investigated by Pratt and his statistical collaborators as reported in a recent issue of the JOURNAL. It was shown in that research that the subject was responding in his ESP card calls, not to a single target but to a three-symbol group or pattern of calls. A further observation of like significance for the unitary nature of extrasensory perception will be reported by Esther Foster. She has found, from her analysis of ESP test data based on the use of playing cards, that the subject made his extrachance hits preponderantly on the card as a whole, rather than on number, suit, and color separately.

Yet while ESP does appear to be a perception of patterns or wholes, in another respect it is limited. There are degrees of coverage or completeness in perception, perhaps best dscribed as "partial perception." This line of thought began first with a discussion of the comparison of positive and negative deviations by

Thouless in 1935 and it was brought up again in 1938 by A. A. Foster in connection with his suggestion for an ESP quotient. If Foster's quotient is used for computing a comparative measure of ESP scoring in high-aim and low-aim experiments in which the subjects' scores give approximately the same deviation below the mean in low aim as they did above the mean in high aim, the low aim gives a quotient four times as large as the high aim. The point brought out (besides the need for a correction for the quotient) is the fact that when the same deviation is obtained for high and low aim, this result is not due to the type of complete perception to which we are accustomed in the senses under normal conditions. Rather, more perception is needed in calling a card correctly in a high-aim test than in merely saying in a low-aim test that a certain card is not a star or some other specific symbol.

This "partial perception" is, however, quite in line with the "pattern perception" concept of ESP already discussed. When, for example, in a case like that of reinforcement (mentioned above) the ESP impression of one target seems to be reinforced by the impression of a nearby target of the same denomination, we must assume that the perceptual process is one that needs support—that is, it is not an all-or-none type of activity.

Here again, as in the case of the personality studies, it may be not ESP itself but other elements that are involved in the complex operation of a response to the target pattern. But if step-by-step the parapsychologist continues to get back toward the fundamental operation, he is making progress.

* * * * *

In the rapid advance parapsychology is making into the finer, more technical branchings of its subject matter, there is great danger that many workers will lose sight of the general place of psi in the personality as a whole. The orientation value of keeping this problem in perspective justifies its designation as a third psychological front. Moncrieff, in his recent book, *The Clairvoyant Theory of Perception,* has built on H. H. Price's earlier suggestion (see *Philosophy,* October, 1940) that clairvoyance may be "our normal state," with the senses acting as selectors to exclude all clairvoyant impressions not biologically acceptable. Moncrieff proposes that clairvoyance or "direct perception" is the explanation of visual per-

ception, both the binocular vision of man and the compound vision of the insect world. Thouless and Wiesner among others have proposed psi as playing a fundamental role in the normal functioning of the individual personality, a role fundamental both to sensory and to motor operations.

Help on this larger question has recently come too from a related quarter, though one not commonly thought of as closely associated. We refer to the acceptance by the neurophysiologist, Eccles, of the suggestion that psi phenomena may furnish an advantageous lead to the solution of the problem of the relationship between thought and the brain. In his recent article in *Nature* Professor Eccles integrates the findings of parapsychology with the approach of the neurophysiologist in attacking the mind-body problem. This cooperative gesture is the more encouraging in that it comes from the other side of the psychophysical gap.

There are a number of developments of neurophysiological character that may well converge later into a major research program if the proper encouragement is given. Findings from exploratory studies of the effect of drugs on psi, comparisons of tests before and after electric shock and other treatments of mental patients, and a few related lines lead us to hope for the emergence of a new research approach, a cooperative one that includes the rich resources of neurophysiology and the exploratory techniques of psi.

Nothing has been said thus far about the prospect in psychokinesis. The fact is, there is much less to say than there is concerning ESP. A wide range of explorations has been made in recent years using different methods of testing, and many of these efforts show results of promise. As yet, however, this promise cannot be adequately verified. It rests in nearly all instances upon a single piece of work. Any one of a dozen of these new techniques may at any time, of course, turn out to be the setting of a new course in the PK research. All it needs is to furnish a more effective way of producing results than anything hitherto. As is usual in new lines, much of the effort in PK research has been too widely scattered over the many different types of approaches and methods. If all that is now known could be put into a few well planned experiments on the problem of discovering how PK operates, this branch of psi might take the lead in clarifying the psychology of psi.

* * * * *

No prophetic insight whatever is needed to foresee a great deal of biological emphasis in forthcoming developments in parapsychology. It should, of course, be just a matter of time until any investigation begun on a living being finds its way of necessity back to biological fundamentals. Claims have already been made for evidence of psi in animals, and in some instances with some experimental support; at least there is the work of Bechterev with dogs and the report on the experiments with the horse Lady by J. B. and L. E. Rhine.

In general, however, the question of the biology of psi phenomena has been overlong neglected. It would seem that a research field must reach a certain stage of maturity and organization before the time is considered ripe for getting down to fundamentals. Such a whole new subdivision as the research on psi in animals could not have been successfully opened up at a much earlier stage. The problems were there but parapsychological interest had not sufficiently matured to give them due priority.

Yet even at the present stage of the research on the question of psi in animals, tentative though it be, a wide range of biological problems is pressing for solution. To begin with, there is the general question of whether psi may be found in species other than man, and if so, which ones, to what extent, and what types of psi. There is the related question of evolutionary origin, as well as that of individual heredity. Arising, too, out of the comparative study of psi in animals there should come some light on the relation of psi to the directive forces of the animal organism, especially those of the nervous system. There can be no doubt that if suitable test procedures can be devised and the investigations made in a way adapted to the needs of the organism and the purpose of the experimenter, this range of investigation will be highly rewarding.

To what extent the unexplained direction-finding performances of animals will require ESP for their explanation may be a matter for lengthy research. But such performances as navigation over long distances in animal migration and the puzzling capacity of many species to make successful returns in homing experiments are sufficiently challenging to warrant a vigorous investigation. A degree of acquaintance with the front lines of zoological research in problems of homing encourages us to think that the hypothesis

of psi in animals will not be a subject of undue controversy, but that objective investigation and the discussion of results in the light of the experimental findings can be carried on with proper scientific dispassionateness. Much of the material already available very strongly suggests the need to bring the ESP hypothesis into consideration along with the others. The designing of future experimental work should take it into account, and we anticipate that many biological investigators will adapt their research design to include it.

It seems safe to predict that the problem will not long remain a question of whether psi functions play an important part in animal behavior, but rather a question of how far they go, what part they play, and what differences in the organism facilitate greater exercise of extrasensory perception in the simpler organism than in the more complex. While the concept of psi in animals is still a hypothesis, it is no longer a merely speculative one—that is, one without support of fact. Rather, it is a principle already established as active in one species and now being considered in others. This fact sets it above the more speculative hypotheses current in discussions of distance orientation.

Biology as well as psychology has had its controversy over dualisms. The biologist's peculiar "allergic reaction" has been directed to anything suggesting a hypothesis of vitalism. This issue, which probably only needs logical analysis for its banishment, does not concern us here unless someone supposes that by a psi factor is meant something to which the biologist is reactive, as he has been to a vitalistic hypothesis. If a psi factor is a determinant that is just as lawful as anything else in biology and just as "natural" as physics itself—though these natures are not identical—there should be nothing left to fear, unless it be the danger that science will continue to overlook the important part this factor may be playing in the biological world.

With a sufficiently detached perspective to bring all human life and culture into one panorama, it is not difficult to see that the unbridged gap between current popular beliefs about the nature of man (for example, theological beliefs) on the one side and strictly scientific knowledge on the other represents an enormous block to progress in human relations today—one that could be disastrous to the race. If there were space for it, we could choose many of the

most important problems of human life to show this yawning abyss of intellectual estrangement between our institutions and disciplines. Let us take only one.

Most people around the world today hold, and to some extent guide their lives by, a traditional belief that there is something in man that biologists do not know anything about. Obviously anything that influences so many people is of the greatest social importance, whether it is error or scientifically unrecognized truth. This belief is the one that holds to something indestructible, hence nonbiological or immortal, in the human person. It would appear that only by the routes being opened up between parapsychology and biology can there be a successful investigation of the basis of this cultural belief by the methods and standards upon which biology itself has come to rely. Only thus can there be a closing of the gap, an integration of knowledge with those cherished values by which most men have been guiding their lives. It is only by understanding life in all its major relationships that we can determine what to live by and what there is to live for, and do so with the larger degree of enlightenment that intelligent men everywhere demand.

* * * * *

Parapsychology has developed a need for another dimension of investigation, namely, a psychophysical one in the broadest sense of that term. Psi has produced a problem that is as much one for physics as it is for psychology or biology. Psi does not function in such a way as to be fitted into principles claimed by the science of physics today, but it does involve psychophysical interaction, a causal interrelation between subject and object. Likewise, if there is any continuity (interaction, causation) between normal subjective experience and brain activity, the problem of how experience can initiate a physical operation in the brain arises for physics, just as on the other side the question of how the physical state of the brain can generate thought should challenge the psychologist.

But whether for physics or for psychology or for both, a new attack on the mind-body problem has been made possible by the isolation of psi. It offers another subject-object relation which, however complex it may be, is still simpler than the thought-brain relation in the living individual. At the moment there is the prospect that physicists are more likely to take the lead into this psycho-

physical no-man's land than are the psychologists, but both will, we hope, close in together.

One reason for raising the question of who will take the responsibility for investigating this basic problem of the nature of personality is that physics, with its first choice of the resources for research could, if it recognized its stake in the problem, greatly accelerate the rate of progress toward its solution. Yet neither the problem of the nature of psi nor that of the thought-brain relation is a one-sided one—or, for that matter, a two-sided one—for neurophysiology should be as much a participant in the program as either physics or parapsychology.

No one but the physicist, however, can furnish the assistance needed in thinking through the problems of energetics or causation associated with the production of effects that elude the sense organs yet somehow worm their way into consciousness, that do not conform to the familiar rules about time and space and yet interact with energy systems that do, and that produce intelligent effects, purposive in character, without conscious register (except through their end products). It may well be that if in some future conference sufficiently expert and venturesome thinkers from physics, neurophysiology, psychology, and parapsychology can be seated around the table where these fragments of findings are assembled, some further outlines of the puzzle may be discerned clearly enough, at least, to justify still further exploratory efforts to close this treacherous crevasse of ignorance concerning man's own nature.

On the somewhat more empirical level of psi experimentation physical scientists such as Rush, McConnell, and others (including engineers like Forwald) will probably turn out new findings that can help the theoretical physicist with the psychophysical problem he must eventually help to tackle if it is to be solved.

There is sure to be a continued attack—for it is already planned—on the limits of the independence of psi from time, space, and mass relationships. More than that, there will be studies of more than one of these physical conditions taken together, with a combination of distance tests and precognition, of tests of PK with an element of precognition, and still other linkages. Beginnings have been made already.

Experiments in precognition must be conducted with predictions

extending for a longer period than the one year ahead to which Miss McMahan has already carried them, and further PK tests are needed to push to its limits the question of the effiect of mass on PK. Either there *is* a limit or there is not; if there is any relation of mass to PK at all its discovery would be of extreme importance. The same is true of distance and time in relation to psi. Surely the near future will see some of these fundamental issues cornered and settled.

* * * * *

Other trends in the future of psi research are discernible, but their outlines are less easily traced. Questions of application, bearing, value and the like are becoming more urgent than ever, as something of the significance of psi begins to dawn.

The time must come, of course, though it may be far off, when application of the psi research findings to practical problems may be expected. At present the most plausible claim of a practice that looks as if it involves psi is the old art of dowsing, or water divining. There is at present a great deal of popular interest in dowsing, largely aroused by Kenneth Roberts' book, *Henry Gross and His Dowsing Rod,* and it is possible that this widespread interest may lead to a more thoroughgoing investigation of dowsing. A more decisive research program is needed than has ever been tried as yet, one that can combine scientific reliability with conditions as favorable as possible to the dowser's hypothesis and the elusive psychology of psi.

Application of the results of the psi researches to the world of human relations, however, needs to be delayed no longer. There is some prospect of having this fact realized before too long (or too late). The spread of communism has made it increasingly important for western man to look not only to the dryness of his powder but to the state of his ideological defense as well. The latter, in the long run, is paramount. Consequently there is a certain sharpening of interest in the question of what man is—the question of whether the communist kind of materialism, or any other kind, gives a valid picture of man. It should be reasonable to predict that as this world clash of philosophies of man becomes more acute there will be a turning to parapsychology for the kind of research that can test the materialist hypothesis of man and that can cope with

the issue of volitional freedom and with the more positive properties in man that are lumped off as "idealistic nonsense."

This creation of interest in parapsychology from the pressure of present social needs is a wholesome one. It promises to be a growing trend as a part of the task of meeting the problems confronting life today. In its origin parapsychology was once largely concerned with listening for the footfalls of another world. Now it is coming down to earth, at least with one foot, though not relinquishing any of its problems or its phenomenology or its values that are verifiable and real. The only difference is that parapsychology is now achieving more of the realism and reliability that make an inquiry scientific.

On the whole, then, the outlook in parapsychology, while it could be better, is good.

J. B. R.

# CONVICTION AND ASSOCIATED CONDITIONS IN SPONTANEOUS CASES

By Louisa E. Rhine

**ABSTRACT: A large collection of reports of spontaneous psi (psychic) experiences was examined to see in what percentage there was a definite feeling of certainty about the validity of the experience. In more than half of the 1600 case reports in the Duke collection the percipient experienced definite conviction. The suggestion therefore is that something takes place in spontaneous psi that occurs only rarely in the laboratory tests. Further, about half of the cases were dream experiences; and of these approximately only one-third showed conviction, while about two-thirds of the waking cases did. Of all the spontaneous cases approximately a third were precognitive. These showed a lower percentage of conviction cases than the non-precognitive psi experiences. Accordingly the lowest combination, so far as conviction goes, was the precognitive dream, and the highest the nonprecognitive psi experience in the waking state.**

**The Duke collection had already been broken down into subdivisions identified with current problems in psi research. They were problems connected with the major types of psi and those connected with the manner in which psi is experienced and the extent of knowledge conveyed in psi experiences. These various subdivisions were examined separately for the percentage of cases showing conviction, to see whether it seemed to be associated with any of these other features. Large differences were found. For example, cases that were highly symbolic apparently carried little conviction. Also there seemed to be no tie-up with the amount of detailed information the experience conveyed. A meaningful experience with a minimum of detail was apparently more likely to carry a feeling of certainty with it than a vivid photograph-like picture. Next to this group of cases the highest conviction rate was that of the "blocked" cases in which there was a simple significant compulsion to act, or a meaningful emotional state. Some explanatory suggestions are considered in the discussion. The high frequency of conviction points the need for a fresh attack on an aspect of psi phenomena that has been entirely missed in past experimental work. The differences in conviction between certain case types suggest that there are conditions to be discovered that should lead to control over this feeling of certainty.**

**Dr. Louisa E. Rhine is a member of the staff of the Parapsychology Laboratory at Duke.—Ed.**

## Introduction

Parapsychologists have long and unsuccessfully attempted to find a method of gaining conscious control of psi processes. The problem has been clarified somewhat by the realization that these processes are unconscious, but no way of raising them to the conscious level has been discovered. In laboratory tests the subjects can seldom distinguish when they are making high scores from when they are making low ones. There have been rare occasions, however, when a subject has felt especially certain of the correctness of

a given call and when the call did prove to be correct. It would seem possible that such a flash of certainty, or conviction, might indicate an element of consciousness in the experience. If so, a study of such instances should yield some hints as to how the unconscious processes could be made conscious.

Although conviction is rare in laboratory tests, it is reported frequently as an element in spontaneous psi experiences. Nearly everyone has heard tales of some person who experienced an awareness of a significant event happening at a distance, and who believed it with a strength of conviction that could not be altered by rational persuasion. And of course in such reports later information bore out the accuracy of the impression. A large number of instances of this kind have been reported to the Parapsychology Laboratory and records of them are on file there. In view of the strong conviction felt in such cases and the lack of it in experimental situations, it seemed likely that suggestions as to its nature, and that of the factors producing it, might arise from a study of it in spontaneous cases. That study was undertaken because of the urgency of the problem. The study broadened as it went, for it soon appeared that several factors in the total case material might be related to conviction. The present paper presents the results of the study. It was necessarily an exploratory undertaking, but it may serve as an example of the use to which a case collection can be put. However, it is not to be considered the only way such material could be handled.

Before presenting this first analysis made upon the Duke case collection, it will be necessary to say something about the standards used in selecting cases, and about the objectives and system of classification.

The general attitude and objective of case studies was that suggested by J. B. Rhine.[1] The objective as he outlined it was to use case material as a source of suggestions for research, but not as proof of anything. Cases were therefore selected with the idea of obtaining as wide a range of suggestions as possible and no material was excluded that could contribute to that end. Cases were acceptable, therefore, with or without supplementary valida-

[1] "The Value of Reports of Spontaneous Psi Experiences," *J. Parapsychol.*, 1948, **12**, 231-35.

tion if they seemed to be contributed in good faith and by apparently sane individuals.

The important criterion for acceptability was that the experience should have in it an apparent psi factor. Since the confirmation of an experience by other persons was not stressed, many cases could be included which otherwise would have had to be rejected, as, for example, certain dreams and other experiences of introspective nature which could not have been vouched for in any case. The value of the collection as a possible source of suggestions for research was thus greatly increased. Of these cases, however, it can only be said, "These things people report," but not, "These things happen."

Although the collecting of cases at the Parapsychology Laboratory is still going on, a group of 1,600 has been considered a first unit. It is the basis of the present study. In classifying these cases the objective was so to file the individual ones that they would be readily available for future reference by investigators with some specific problem in mind. But any attempt at classification based on future need must necessarily be tentative because, of course, all possible angles of study which might later be made could not be foreseen. However, there are many major problems which seem likely to engross researchers for some time to come, and these were taken as the basis for the classification of the cases. An outline of them was drawn up, the portion of that outline pertinent to the present paper is given below.

Partial Outline of Problems Under Which Spontaneous Cases Were Classified

Section I. Problems bearing on the nature of the different types of psi phenomena.

- A. Precognition
- B. Telepathy
- C. Clairvoyance
- D. Psychokinesis

Section II. Problems connected with the way psi is experienced

- A. Manner of experience
  1. Dramatic
  2. Symbolic
  3. Photographic

B. Extent of knowledge conveyed
   1. Blocked
   2. Fragmented
   3. Meaningful
   4. Complete

Section I of the above outline covers all the problems relating to the types of psi as such. However, even these types do not present problems which are of equal urgency at the present time. Therefore, the actual classifying of cases into the four types—Precognition, Telepathy, Clairvoyance, and Psychokinesis—was not completed uniformly, but adjusted rather to conform to immediate need, as stated later. Working definitions of psi types were used but they of necessity did not take account of fine points of discrimination which might in themselves be the basis of future study. Instead, only common usage definitions were employed in making type classifications. Thus all cases that showed a time discrepancy between the experience and its later fulfillment were considered as belonging under Precognition; all in which it seemed as if the subject's experience was based on the thought or knowledge of a second person, under Telepathy; all in which the subject's experience seemed at least superficially to be a response to a thing, event, or situation rather than a thought, belonged under Clairvoyance. And all cases in which a physical effect was reported were defined as Psychokinesis.

Section II covers problems concerned with the way in which psi is experienced regardless of its type. Even within any one type the psi impression may come in different forms, and similar forms may be recognized in different types. The main groups of problems of this nature noted in Section II were those having to do with the manner of the experience and those having to do with the amount of knowledge conveyed. These distinctions will be described and illustrated more fully later. Here it is only necessary to distinguish the basic difference of character between the problems of Section II and those of Section I.

Since the problems of Sections I and II were different, the same cases could have been used in both sections. The same is true of the subdivisions under Section II. All the cases of that section

could have been classified both in the (A) subdivision, relating to manner, and in the (B) group relating to amount of knowledge. However, with minor exceptions noted later, the policy followed throughout was to use each case but once, placing it in the category to which it seemed it might make the greatest contribution. In this way it was hoped that the groups would be mutually independent, should such independence prove desirable in the comparisons which might later be made.

As stated before, the classification of case material into the four types under Section I has not yet been completed. At the time of writing this report only the Precognition and Telepathy groups had been isolated. The former included those instances which showed a definite time discrepancy. If the situation in a given case was such that it was impossible to tell whether or not the experience did occur before the event, even though it was likely that it did, the case was considered indeterminate and not placed under Precognition but reserved for other use. Similarly only those cases were classified under Telepathy in which the thought or knowledge of another person seemed clearly to have been the cause of the experience. If it was an apparently indeterminate case, like the indeterminately precognitive cases, it was reserved for use elsewhere.

The rest of the cases, which were mainly those of clairvoyant type, but also the rarer psychokinetic ones, and all which were indeterminately precognitive or telepathic as mentioned above, were left in a group labelled "Undifferentiated." This group seemed especially rich in suggestions for the problems of Section II. The Undifferentiated cases were therefore classified into the subgroups of that section because it happened that those problems were of greater current interest than any concerning clairvoyance or psychokinesis as such.

It had been noted when forming the Precognition and Telepathy groups that many cases in those groups, but especially in the Precognition group, gave suggestions on the problems of one or the other of the Section II subdivisions. Although these latter cases had the time discrepancy which made them precognitive, they also seemed to give many possible suggestions as to the manner of the experience or the amount of knowledge shown, just as did most of those of the Undifferentiated group. Therefore, the cases which

seemed particularly suggestive in either of these ways were kept separate from the main Precognitive group. (The same could have been done for Telepathy, but had not been done at the time the collection was examined for the conviction survey.) The separation is mentioned here because it figures in the analysis to follow and, like the rest of the original case classification, was made before the present study was projected. That fact may give a certain added value to the comparisons between groups which will appear in the course of this report.

### The Survey of Conviction in Cases

Although it is not essential to the present report, a word about the initial approach to the conviction survey may be of interest. It was first considered possible that a study of a large sample of cases showing a high order of conviction might yield some definite suggestions as to the conditions producing such a feeling. Accordingly, about a hundred examples were selected and studied for any hints that might be given as to how the subjects "knew" that the impressions they got were true. A few typical quotations from these selected cases will be given as illustrations.

* * * *

My mother dreamed that something had happened to my brother. She was sure he was hurt. She went 90 miles to San Francisco and found him in a hospital, his arms and hands burned and bandaged.

* * * *

I was a janitor at the high school. One night I awakened from the most realistic dream that the school was on fire. I at once dressed and hurried to the school and found a fire started that in a few more minutes would have destroyed the building.

* * * *

I was visiting my sister in another city when I suddenly felt that my little girl at home had burned herself very severely. (She had, as the mother found when she hurriedly returned home.)

* * * *

While attending church my mother became faint and father had to help her out during the sermon. She said, "Oh, Alex, it is John." (That was her brother. Later they received the announcement that he had died at that time.)

* * * *

It soon became clear from examples like the above that these cases could yield nothing on the point in question. In spite of the unquestioning belief expressed, no individual gave any hint what-

ever as to why he believed his hunch. The strength of his conviction is attested to in many cases by the action he took as a result of it, but in every instance, he "just knew," but had no idea how he knew.

Although this survey yielded no clues to the process in question, the frequency of occurrence of cases of this type, as well as the strength of conviction expressed, stimulated interest in the further examination of the factor of conviction. It seemed probable that a study of cases which did not show it, as well as of those which did, might yield some suggestions. Accordingly a broader analysis was undertaken, one that included the collection as a whole.

### Classification of Cases on the Basis of Conviction

The 1,600 cases in the collection were divided into two groups: those that did not show conviction (NC) and those that did (C).

The NC group included:

(1) All cases in which a subject could hardly have taken his experience as significant at the time of its occurrence, but in which he recalled it later when subsequent events made it seem meaningful. Many precognitive dreams, such as the following one, were of this type:

> One night I dreamed I saw a certain oil tank of which I had charge blazing with fire. The next day I had just gotten home and was washing up for lunch when my wife looked out the window and said, "There is something afire across the road." I started on the run and found the tank on fire. Other men soon arrived and by fast work we soon extinguished it. Then on my way back home I recalled the dream of the night before.

(2) All cases in which the subject was confused or worried about the possible significance of his experience, but undecided as to whether or not it might be meaningful. In these cases the subject did not dismiss his experience as meaningless but neither did he feel certain about it. The following is typical:

> At the beginning of World War II my husband had a nervous breakdown and was in a nursing home about 50 miles away from home. We wrote to each other daily, but one evening about 8:00 p.m. I was sitting alone in my drawing room in the flat when suddenly I became very nervous and restless. My agitation increased. I could not bear to sit still and I paced up and down. Finally I went out into the hall and found myself staring at the telephone. A longing came over me to ring up my husband at the nursing home. This I would

not let myself do as the authorities asked us not to use the telephones unnecessarily to avoid congestion of the lines in case of air raids. In great distress I walked up and down the hall by the telephone and then gradually after about half an hour I became easier and went back to the sitting room. When my daughter came home I told her about it saying, "And I shall be glad when I get your father's letter tomorrow and hear that he is well." The next day I got two letters, one my usual letter and the other addressed earlier saying, "I feel very homesick. Do telephone tomorrow night and I will be waiting by the telephone from 8:00 to 8:30 p.m." This letter he had hoped I would receive by evening post the day before. The following day, 36 hours after my anxiety, I received another letter, "Why did you not ring me up? I waited by the telephone from 8:00 p.m. until 8:30 in such agony of mind hoping to hear your voice."

The Conviction (C) group also was made up of two kinds of cases:

(1) Those of the type mentioned earlier, in which the subject demonstrated his belief in the meaningfulness of his experience by taking appropriate action, as in the following example:

I owned a house about seven blocks from where I lived. One night about one o'clock the fire whistle blew and it woke me up. I jumped out of bed and started dressing as fast as possible. Of course my mother asked what in the world I was going to do. I said, "Mother, my house is on fire. I am certain that it is." Well, she told me how foolish she thought I was but she couldn't stop me. I ran out, jumped in the car and drove there as quickly as possible, and just in time to see my garage burn to the ground. I hadn't had a dream that it was on fire but the second the whistle blew I knew it was. The strange thing was that I never paid any attention to fires or fire whistles either before that time or since then. Can you explain that? I had no doubt whatever in my mind that it was my house, and all my mother's arguments couldn't change me. However, it was the garage instead of the house that burned.

(2) Those in which the situation was such that no action was called for, but definite belief was indicated. This proved to be a large group, a typical case being:

Mrs. A., who had not been well, wakened on the morning of November 18 sobbing and crying, saying nothing but "Jack is dead. Jack is dead." Her husband was unable to calm her and finally called the physician, who gave her a sedative.

Mrs. A. went along until the morning of November 23 when the same thing happened. Again her husband was awakened by her sobbing and crying out, "Jack is dead. Jack is dead." Again he was unable to calm her and called the family doctor who gave her a seda-

tive and advised a psychiatrist. Before reaching a psychiatrist the husband had a letter from Jack written November 15 and stating all was well. His wife was glad to hear the letter, but it did not change her mind. She still insisted that Jack was dead.

That evening a telegram was delivered telling of Jack's death on November 17 in Hawaii. When told of it Mrs. A. replied, "I knew it all the time but you wouldn't believe me."

The above case is, of course, one of strongly expressed conviction. In many others as reported, it was more difficult to judge the strength of conviction that was felt, but the policy followed in making these judgments was to rate as NC rather than C, cases in which the degree of belief was indeterminable so that the characteristics of the C group would be reliably those of conviction.

Using the above criteria of judgment the 1,600 cases of the collection were divided into the two groups as follows:

| | |
|---|---|
| Non-conviction (NC) | 760 |
| Conviction (C) | 840 |

These figures, which show conviction cases as composing more than half the total, are interesting in view of the fact that under laboratory conditions, conviction occurs so rarely. Very probably in reports of spontaneous experiences selection in favor of high conviction cases occurs. Experiences of that kind are more impressive to the individual, would be remembered longer, and would be more likely to be thought worth reporting. It also may be that many unimpressive spontaneous cases of psi occur that are never even recognized, much less reported. And there may have been errors in judging conviction cases. But whatever may be the actual proportion of cases of conviction to those of non-conviction in reality, the point here is that both types occur in this collection and in roughly equal numbers, so that study and comparison are possible.

### Conviction in Dream Versus Waking State

The first step taken in studying the occurrence of conviction was to find what relationship might exist between it and sleep or waking. Spontaneous experiences are reported as occurring during various degrees of wakefulness. However, in the collection of cases to be surveyed, all but 156 of the total 1,600 occurred either when the subject was in what he definitely identified as the waking state

or in dreams. However, since introspective testimony is notoriously fallible as to wakefulness under conditions of repose, experiences occurring when the subject was lying down but thought himself awake were rated as doubtful. Cases so judged make up most of the 156 above and are excluded from the following survey. For the survey only those instances that appear to have occurred either definitely in the waking state or definitely in dreams are included.

The results of the survey are shown in Table 1. This table shows the four-way distribution of conviction (C) and non-conviction (NC) for both the dream and the waking cases. The percentage of the total which each figure of the four-fold distribution makes, is also shown in this and following tables, in order to facilitate ease of comparison of relative values. The type of distribution of figures in these tables would, of course, suggest the use of the chi square as a convenient statistical method of evaluation of the significance of the distributions. However, the use of statistical methods on samples collected as these necessarily were would obviously be of very limited value and could easily introduce a seeming, but false, accuracy. If the chi square was computed for each value many would be found to be highly significant, especially Tables 1 and 2. But it seems better to rely upon inspection instead for whatever trends may be noticeable, since the entire objective of the survey is to obtain suggestions for experimentation, rather than to prove any point.

The figures of Table 1 have two points of interest. One is an apparent association between conviction and waking experiences, for the percentage of waking experiences that are believed is much greater than that of dreams believed. In fact, the ratio of believed

Table 1

Conviction in Dream Versus Waking States

| | Number of Cases | | Percentage of Total | |
|---|---|---|---|---|
| | Non-Conviction (NC) | Conviction (C) | Non-Conviction (NC) | Conviction (C) |
| Dream..... | 481 | 209 | 33% | 15% |
| Waking.... | 215 | 539 | 15% | 37% |
| Totals.... | 696 | 748 | 48% | 52% |

to unbelieved waking experiences is more than two to one, while dreams that are believed occur only in a proportion of about one to two. The second and perhaps greater point of interest is the fact that the minorities are as high as they are. Fifteen per cent of all cases are dreams that are believed, and the same percentage of all cases are waking experiences that are not believed. If it might seem obvious that waking experiences should be more convincing than those occurring in dreams, still it would not be obvious why there would be so many atypical instances, that is, dreams that are believed, and waking cases which are not.

In an attempt to see if the above association between waking and conviction on the one hand and dreams and non-conviction on the other, were general in distribution or were concentrated in some particular group of cases, the data shown in Table 1 were broken down into their constituent parts, the previously formed groups of cases under Precognition, Telepathy, and the large Undifferentiated group, the nature of which was explained above. The results of this breakdown are shown in Table 2. The distributions are given and also the percentage of the totals of each of the three groups.

In each group of Table 2, the percentage of conviction cases in dreams is lower than that of the corresponding waking group. In

Table 2

Conviction in Dream Versus Waking States in Different Types of Psi

| | Number of Cases | | Percentage of Total | |
|---|---|---|---|---|
| | NC | C | NC | C |
| 1. Precognitive—539 Cases | | | | |
| Dream | 293 | 104 | 49% | 17% |
| Waking | 66 | 130 | 11% | 23% |
| Total | 359 | 234 | 60% | 40% |
| 2. Telepathic—188 Cases | | | | |
| Dream | 43 | 22 | 23% | 12% |
| Waking | 45 | 78 | 24% | 41% |
| Total | 88 | 100 | 47% | 53% |
| 3. Undifferentiated—663 Cases | | | | |
| Dream | 145 | 83 | 22% | 12% |
| Waking | 104 | 331 | 16% | 50% |
| Total | 249 | 414 | 38% | 62% |

each group also there are atypical cases of dreams which are believed and of waking experiences which are not, ranging from 11 to 24 per cent of their respective totals. These figures show that the trends noted in Table 1 are general and not caused by some special group. The suggestion therefore that waking cases tend to be believed in greater proportion than do dreams is consistent throughout. However, the suggestion is only that there may be a relationship between waking and conviction, or dreams and non-conviction, but it is not suggested that one is essential to the other. There are too many atypical instances for that to be the case. Therefore the waking state would appear to be only a factor which favors conviction, but not a causal one.

Another factor affecting conviction may be inherent in the type of psi involved, for the figures of Table 2 show that the proportion of conviction to non-conviction cases is not uniform throughout the groups. In the Precognition groups less than half of all cases, 40 per cent only, are conviction cases. In the Telepathy group, slightly more than half, or 53 per cent, are that, while in the Undifferentiated group 62 per cent are conviction cases. If 50 per cent conviction were taken as an average for comparison then the Precognition group would have to be considered as one showing a low conviction percentage, Telepathy about average and the Undifferentiated relatively high. Thus there is a marked trend toward higher percentages of conviction cases among the groups as they happen to be arranged in this table. The difference between the most extreme groups, the Precognition and the Undifferentiated, is large enough to be decidedly suggestive of some fundamental differentiating factor, which works either to depress the conviction rate in the Precognition cases or to raise it in the Undifferentiated ones. The rates in the Telepathy group coming between the two others may of course be as indicative of some special characteristic of telepathy as that of the Precognition group is for precognition. But in a survey such as the present one the emphasis must be on the most outstanding effects only, and therefore the contrast between the conviction percentages in the Precognition and the Undifferentiated groups is more interesting than that between them and the Telepathy group. Nevertheless, the suggestion is given that there may be a connection between conviction rate and the type of psi involved,

and that the rate is especially low in the Precognition group in contrast to that in the Undifferentiated, even though the latter, as explained earlier, is not a pure type group. But it should be noted that this comparison, too, shows atypical minorities in each group. Therefore, whatever factors there may be that are inherent in psi types affecting conviction, they, as well as the state of consciousness, apparently have only limited, not complete, causal effect.

## Conviction and Manner of Emergence of Psi Data Into Consciousness

The next step in probing for relationships between conviction and other factors in psi cases was suggested by certain characteristics that are likely to be noted by anyone who reads large numbers of accounts of spontaneous experiences. The impression is easily arrived at that in spite of the great diversity of detail reported, there are many underlying similarities even in cases that superficially may appear to be very different. These fundamental similarities lie in the method or manner by which psi impressions reach consciousness. It would seem that there are only a few basically different ways in which this occurs and that the one preferred, for whatever reason, is rather consistently followed (although combinations and mixtures occasionally occur). It seemed worth while to find out if the occurrence of conviction was affected by any of the more obvious variations in manner. Those most suitable to the purpose seemed to be: (1) Dramatic, (2) Symbolic, (3) Photographic. They are illustrated below but a word here on the naming of groups is in order. The names selected for the three groups in this section, and also for the four in the section to follow, are single words which express as far as possible the distinguishing characteristics of the groups. However, it is not easy in each case to find a word that is suitable from all aspects, and therefore the one chosen must be considered only an approximation to the complete definition of cases it covers.

(1) *Dramatic.* In cases showing this characteristic, the incoming ESP knowledge seems somehow to be acted out in a way that the conscious mind can interpret. Various degrees of complexity occur in different cases. Of the three following examples, the first shows a relatively simple dramatization.

My brother was in the Navy and stationed nearly 1,000 miles from where I lived. I had had no hint from him that he was to come home at any time previous to the incident that I am going to tell you about. This is what happened. Early in the morning I distinctly heard someone who sounded like my brother call my name twice. I was so sure that I heard it that I looked out the window to see if he were on the street calling to me, but no one was there. I said to the woman I boarded with, "I'll bet you a dollar to a doughnut that my brother will be home within two days." She didn't believe it, but since I've had these premonitions before I knew it was true, so Wednesday night I took a bus home to the city and who do you suppose was there? Of course it was my brother. He had arrived at 4:00 a.m. that morning here in Albany and had taken a taxi home. Now I had no reason other than this whatever you call it, to believe that he would be home on leave, and he didn't know himself until a few hours before.

The following is a case considered to be of more complex dramatic type:

A few years ago I dreamed about the physician who had attended my brother on a few occasions years before. We had not been talking about him or his family, neither had I thought of him in all the intervening years, but on this particular night I dreamed that mother and I went to a funeral home. As we entered, some woman approached me and inquired if we had come to see Dr. Jones who had passed away. We were quite shocked, as we had not heard of his illness or death.

While standing beside the casket viewing the remains of Dr. Jones, I commented on his quivering eyelids and his apparent efforts to open them, and mentioned it to the attendant who informed me that a new enbalming fluid was responsible.

A day or so later when I read the morning papers you can imagine my shock and surprise to read that Dr. Jones had been a patient at the hospital for about three weeks with sleeping sickness. I told my family about the dream that evening and made the statement that he would not recover. Some time in the next few days he passed away.

This is an example of the apparitional type:

Having recently read your article I wanted to pass this true experience along to you. Last July my uncle was lying fully awake in bed when he says he saw his eldest son standing by the bed, who told him, "Well, Dad, you will have to take care of the children now." This occurred a short time before the radio reported that one of the three bombers on a round-the-world trip had crashed off the coast of Persia. Later they established the fact that it was the one in which his son was co-pilot. A month later they officially declared him dead. He was the father of four small children.

2. *Symbolic.* This method is distinguished from the dramatic,

in that a symbol rather than dramatization seems to reveal the meaning to consciousness, as in the following case:

> As a child I could always tell my mother when there was going to be trouble that would affect our family, because I would have a nightmare or dream of muddy water. After my marriage when I was about 20 years of age, I dreamed the muddy water dream. I saw my mother floating in the muddy water holding a lighted torch high above the water, her hair floating as she was carried helplessly along. Needless to say, I was very much distressed. In a matter of a day or so there was a letter from Dad telling me mother had suffered a stroke, but would recover. I remembered the lighted torch that had comforted me in the dream.

3. *Photographic.* This method is particularly difficult to characterize by a single word, and has such an obvious appearance of simplicity that it is not readily interpreted as a method at all. It seems to be the reproduction in the mind of the subject of a bit of actuality, even though removed from sensory contact. Insignificant detail as well as pertinent fact in some cases as reported are perceived with surprising clarity. A case of this kind may include auditory or other sensory forms besides the visual. Therefore, the name *photographic* is not entirely descriptive. The following is an example of a Photographic case:

> Your article has prompted me to set down one of my many psychic experiences in the hope that it may be of interest and help to you. It is probably a typical occurrence, but for any worth it may be to you, I am happy to relate it. On the night of February 10 I dreamed a not unusual dream of a business associate of my husband's. I dreamed that I saw this person in his basement working with a chair, perhaps painting it or making some repair on it. That part did not seem important. Suddenly he turned pale, fell, and was writhing on the floor, and I could hear him calling to his wife as he tried to crawl up the stairs. His wife and children came to his aid in nightclothes and helped him. There my dream ended.
>
> In the morning I started to tell my husband about this dream, but we were interrupted and I was unable to say more than, "I had a strange dream about Bill last night." I thought no more of it during the day and when my husband returned home that evening he said, "Bill didn't come to work today." When he started to tell me why, I interrupted him and told about my dream. He informed me that that was exactly the sequence of events the night before. Bill had suffered an attack of nervous exhaustion while upholstering a chair in his basement. The night of the dream I had retired at 11:00 and the attack had occurred almost at midnight. It is probable that I dreamed of this as it occurred.

The results of the survey of conviction in the Dramatic, the Symbolic and the Photographic groups are given in Table 3. Two separate sections of data are given for each, the Undifferentiated and the Precognition. There were 292 Undifferentiated cases which seemed to exemplify one or another of the three modes, and 278 from the Precognition group which did so. The data from the two groups are given in separate sections of the table, then the totals which they yield when combined, and finally, the percentage which each figure of those totals represents, on the basis of all the cases combined.

Table 3

CONVICTION IN RELATION TO MANNER OF THE EXPERIENCE

| | Undifferentiated Number of Cases | | Precognition Number of Cases | | Total | | Percentage of Total | |
|---|---|---|---|---|---|---|---|---|
| | NC | C | NC | C | NC | C | NC | C |
| 1. Dramatic—211 Cases | | | | | | | | |
| Dream..... | 49 | 22 | 23 | 11 | 72 | 33 | 34% | 15% |
| Waking..... | 32 | 65 | 4 | 5 | 36 | 70 | 17% | 33% |
| Totals.... | 81 | 88 | 27 | 16 | 108 | 103 | 51% | 48% |
| 2. Symbolic—57 Cases | | | | | | | | |
| Dream..... | 12 | 2 | 14 | 7 | 26 | 9 | 45% | 16% |
| Waking..... | 7 | 4 | 8 | 3 | 15 | 7 | 26% | 12% |
| Totals.... | 19 | 6 | 22 | 10 | 41 | 16 | 71% | 28% |
| 3. Photographic—302 Cases | | | | | | | | |
| Dream..... | 32 | 27 | 126 | 33 | 158 | 60 | 52% | 20% |
| Waking..... | 8 | 32 | 18 | 26 | 26 | 58 | 9% | 19% |
| Totals.... | 40 | 59 | 144 | 59 | 184 | 118 | 61% | 39% |

The first observation on Table 3 is that there is a certain amount of similarity of trend noticeable between the Undifferentiated and the Precognition sections of the table for each of the three subdivisions, Dramatic, Symbolic, and Photographic. Degrees of association vary, but the trends are the same. For example, in the Undifferentiated section of Dramatic dreams only about half as many are believed as unbelieved (22:49) and the proportion is roughly similar in the Precognition section of Dramatic dreams (11:23).

The comparable waking groups of Dramatic cases cannot be compared, however, due to the fact that the figures in the Precog-

nition section are too low. The similarity of trend in the Undifferentiated and Precognition sections is most noticeable in the Symbolic group. In it, the figures for non-conviction are higher than those for conviction in both sections and in both dream and waking groups. Unfortunately, the numbers of cases involved in these groups are too small to be impressive but the fact that the two sections have such similar trends is noteworthy. In the Photographic groups, the dream rates are lowered by the fact that so many of the cases of the Precognition group occur as dreams, and dreams are low on conviction, as already found. The percentages of conviction involved in the two sections of this group are quite different, even though trends are in the same direction.

Although the similarities of the two sections in general are not pronounced, except in the Symbolic group, there is a suggestion that the three groups may each have a characteristic effect on the conviction rate. The suggestion is given mainly by the fact that the Symbolic percentages are so much different from those of the Dramatic and Photographic groups. Of course those groups themselves have different conviction percentages, but the differences are small and may or may not be meaningful. Nevertheless, the possibility that different manners of expressing psi may effect conviction should be noted.

One other observation on this table is that, taking 50 per cent conviction as a standard, all the groups involved are low-conviction just as Precognition in Table 2 was, for here the Dramatic has only 48 per cent, the Photographic 39 per cent and the Symbolic only 28 per cent of all cases showing conviction. Since the low conviction of the Precognition group is reflected in the above totals however, the higher percentages of the Undifferentiated section should be kept in mind. At any rate the Symbolic cases at least are low in conviction and if the small numbers involved have any reliability they indicate that something in the Symbolic method depresses the conviction rate.

### Conviction and Amount of Knowledge

An analysis of conviction in relation to the amount of knowledge conveyed was in any case a logical next step, the groups upon which to base it having already been formed in the original case collection, just as had those reported in Table 3. In addition, how-

ever, the survey of conviction and amount of knowledge was prompted by the thought that conviction might very well be the end result of the acquisition of necessary information. If so, it would occur when that was forthcoming, but be absent when the facts were lacking.

By definition, a psi experience is one in which knowledge not accessible to the senses gets into consciousness. But in spontaneous cases as reported the amount of knowledge of the pertinent event so secured varies. Although frequently an individual may be spontaneously aware of an entire situation, in many others the knowledge conveyed seems to be limited so that he is only aware of part, not all, of the event concerned. Four groups covering the different degrees of incompleteness had been recognized in the original classification of cases. These were listed according to degree of incompleteness to form a gradation from those showing the least possible awareness to those showing the most. Appropriate cases were filed under each. The groups so formed were: (A) Blocked, (B) Fragmented, (C) Meaningful, and (D) Complete. The frequency of the occurrence of conviction in these four groups was now determined to find the degree of association between it and the amount of knowledge. The four groups are defined and illustrated as follows:

(A) *Blocked:* Many instances are reported which suggest that there exists below the conscious level of the subject's mind a suppressed or blocked cognition of a pertinent event or situation in which the essential meaning does not get into consciousness, although an appropriate emotional state is experienced. In these cases the individual seems to have felt or acted as he would have if he had been conscious of the information, which for some reason remains repressed. Naturally the question comes up whether there is an actual relationship between the effect and the event since identifying facts are lacking. The frequency of occurrence of such cases and the similarity of psychological states as reported are the best reasons for tentatively considering them to be cases of psi manifestation. The following examples are typical. The first is an extreme case in which the individual gets none of the essential meaning and does not know even to whom the emotion and compulsion apply.

One day I said to my husband, "I am going to call on Alice S. this afternoon." She was a friend of mine who lived in a different town. I left my husband reading, and when I looked back he waved to me as he always did. I took the train to W. where my friend lived.

No sooner had I arrived at her apartment than I had a strong urge to leave at once, although I had lived there for years and had many good friends there. Ordinarily I would have made several calls before returning home. However, I remained with Alice only a few minutes and then I left. The desire to be at home grew and grew upon me and as no bus appeared at once I started to walk to the railroad station. I met another old friend on the way. She stopped me and was all set for a chat. However, I excused myself saying, "I am terribly sorry, but I must catch the next train." As I neared home the feeling of great catastrophe almost overcame me. When I entered my apartment I found the husband I had left in perfect health a short time before unconscious on the bedroom floor. He never regained consciousness and passed away a few hours later.

In the next case, the person to whom the experience applies is known, but the meaning is not:

My husband Jim and my sister and I were out in our car driving along and I suddenly had that strange foreboding again. I turned around and said to them, "Something has happened to Ralph." He was a good friend of ours. For two weeks he was in my mind as if I had to know, like he was trying to tell me something. Then we found out that he had died about the time I told them."

(B) *Fragmented:* These cases are the ones in which more knowledge of the pertinent situation is shown than in the blocked type, but some of the essential data are lacking, confused, or inaccurate. A great many degrees of incompleteness of knowledge could be isolated. The following example is one in which the details are known, but an important feature, the essential meaning, is lacking.

I was living with my aunt in a small town in Scotland, my father having brought me there and then having returned to Canada. One night I dreamed that a messenger boy had come to the door and I had taken from his hand a telegram. He had pulled out of his pocket a stubby yellow pencil for me to write the answer. I told him there was none, and then came back into the hall where I broke open the telegram. I could not read the message it contained, for a cruel light broke over me and in the illogical manner of dreams I knew that the light was identical with the message and that it was something to be terrified of. I shrank away from it, but still it enveloped me.

On the afternoon of the following day the actual experience happened. It was all there, even to the stubby yellow pencil which the

boy took from his pocket. The cablegram was from my brother saying that my father was seriously ill. Actually my father was dead when my brother sent the message, but he didn't want me to receive the news without some preparation.

In the next case, at least a fragment of the meaning gets into consciousness but the identity of the person involved is mistaken.

Many years ago my husband, our two small sons and I were forced to spend the night sitting in a car, because of its being dark and we had lost the road. During the night we all slept a little at intervals. We talked meanwhile. Talk awhile and then sleep again. In the time of one of those cat naps I had a very vivid dream concerning my husband's family. I saw and talked with his mother who had been dead a few years, and to his brother Arthur who had a wife and two little girls. In the conversation it seemed that all were feeling sorry for brother Arthur, being left with two little girls without a mother, although in my dream nothing actually was said about it. When I awakened I told my husband and small sons of the dream which they will remember to this day. The next morning when daylight came we found the road and continued on our way home. Several miles from town we met my stepson coming to meet us with a telegram for my husband which told of his brother Arthur having been shot accidentally that morning. He had been killed about 8:00 a.m., several hours after my dream.

(C) *Meaningful:* There are many instances in which the essential meaning arrives in consciousness as a complete item without limitations of the Blocked or Fragmented kind. However, although the meaning arrives as a complete unit, details of information are lacking so that there is still a limitation of knowledge even in this type of experience. On the whole, the Meaningful case is one in which the knowledge conveyed is somewhat in the nature of a hunch rather than a picture or reproduction of reality. A typical case is the following:

On the morning of February 11, 1911, I was awakened with the terrible impression that my mother was dead. I was living in North Dakota at the time and she was in Kentucky. My husband made fun of me and said that if anything had happened I would get a telegram. However, as the hours passed I got more and more frantic. I was positive that she was dead. At one o'clock I received a telegram. She had been killed by a fast train.

In this case it should be noted that the impression was complete in that it covered the essential meaning, "Mother is dead," even though it added no information as to the manner of death.

(D) *Complete:* Cases which may be considered complete are those in which both meaning and essential detail are shown. As it happens, such cases are practically always of the Photographic type, as shown in the example given on page 178, and also in the following:

> I dreamed of a fire in a little town in Massachusetts, my birthplace. I had not visited this town in many years. In my dream I noticed the new fire fighting apparatus, the site of the fire station, and I recognized the burning house. I had known this family very well at one time and their name was Wellman.
>
> The next morning I told my entire family just as though I had seen it in a newspaper, "The Wellmans' house in G. burned down last night. Someone will probably send me word." Sure enough, I received the paper from that town with the account of the fire just as I had seen it. Yes, the fire station did have a new location. Yes, the town had purchased new apparatus. Yes, the house was the Wellmans'.

The conviction survey of these four types of cases is given in Table 4. The material on which it is based came from the same two major type groups used in Table 3. In this case there are 272 from the Undifferentiated group and 163 from the Precognitive. The cases involved are all different from those used in Table 3, with one exception. That exception is the group labelled Complete.

As it happens, cases in which complete knowledge of a pertinent event is shown are Photographic in type; but some Photographic cases are incomplete. In these in spite of the amount of detail that becomes known, certain necessary items are lacking so that the meaning is not clear. Therefore all Photographic cases were used in Table 3, but those which were complete were selected from that group and re-used in the Complete group of Table 4. In this table, as in Table 3, the data from the Undifferentiated and those from the Precognition group are given separately and also as combined totals. The percentages of those totals which each figure represents are also given.

As mentioned above, the groups presented in Table 4 are arranged in the order of the increasing amount of knowledge conveyed. The data, however, do not show a similar orderly increase in percentage of conviction cases. Instead, the Blocked group with 83 per cent of its cases believed, has a very high percentage of conviction cases. Next is the Meaningful group with 72 per cent.

Table 4

CONVICTION AND AMOUNT OF KNOWLEDGE

| | Undifferentiated Number of Cases | | Precognition Number of Cases | | Total | | Percentage of Total | |
|---|---|---|---|---|---|---|---|---|
| | NC | C | NC | C | NC | C | NC | C |
| 1. Blocked—143 Cases | | | | | | | | |
| Dream..... | 0 | 1 | 0 | 2 | 0 | 3 | 0 | 2% |
| Waking..... | 14 | 98 | 10 | 18 | 24 | 116 | 17% | 81% |
| Totals.... | 14 | 99 | 10 | 20 | 24 | 119 | 17% | 83% |
| 2. Fragmented—151 Cases | | | | | | | | |
| Dream..... | 37 | 14 | 48 | 17 | 85 | 31 | 56% | 20% |
| Waking..... | 16 | 9 | 5 | 5 | 21 | 14 | 14% | 9% |
| Totals.... | 53 | 23 | 53 | 22 | 106 | 45 | 70% | 29% |
| 3. Meaningful—141 Cases | | | | | | | | |
| Dream..... | 15 | 17 | 13 | 10 | 28 | 27 | 20% | 19% |
| Waking..... | 3 | 48 | 7 | 28 | 10 | 76 | 7% | 53% |
| Totals.... | 18 | 65 | 20 | 38 | 38 | 103 | 27% | 72% |
| 4. Complete—201 Cases | | | | | | | | |
| Dream..... | 30 | 20 | 72 | 15 | 102 | 35 | 50% | 17% |
| Waking..... | 7 | 25 | 17 | 15 | 24 | 40 | 12% | 20% |
| Totals.... | 37 | 45 | 89 | 30 | 126 | 75 | 62% | 37% |

However, since there is no Blocked dream group, these cases rarely being reported as dreams, the waking group of Meaningful experiences only should be used in comparing the relative percentages of these two groups. The Blocked group yielded 116 conviction to 24 non-conviction experiences, which is in a ratio of about 5:1. Meaningful cases show 76 conviction and 10 non-conviction cases, a ratio of about 7:1, and that is the highest rate found in any table. In the remaining two groups, however, the rates are much lower. The Complete group has only 37 per cent of its total as conviction cases and the Fragmented, lowest of all, only 29 per cent. Or, in terms of waking cases only, comparable to those given above for the Blocked and Meaningful groups, Complete experiences of the waking kind are believed in a ratio of 24:40, or about 1:2; and the waking Fragmented, 21:14, or about 1.5:1.

These figures, then, fail to show a relationship between amount of knowledge and the frequency of occurrence of conviction. The finding that much more conviction occurs in Blocked cases in which

no facts are known than in Fragmented ones in which there is partial knowledge does not support the theory. Neither is it supported by the fact that Meaningful cases are believed much more frequently than Complete ones. The only finding consistent with the theory is the fact that Fragmented cases are less convincing than Meaningful or Complete ones. Any explanation for the results of this table, therefore, will have to be on the basis of something other than amount of knowledge conveyed.

* * * * *

The trends noted in this survey are summarized here for convenience.

1. Conviction occurs more frequently in waking experiences than in dreams, but there are many exceptions.
2. The conviction percentages in the Precognition group are lower than in the other major type groups compared.
3. There is a suggestion that differences in manner in which the experience reaches consciousness may affect conviction percentages characteristically.
4. The high conviction percentages in Blocked and Meaningful groups are in contrast to the low ones of the Fragmented and the Complete groups.

In the discussion below, an explanation which could unify the above points to some degree is attempted.

## Discussion

In the foregoing survey, the suggestion was made that conviction might be related to the amount of knowledge conveyed to consciousness. The suggestion, however, was not supported by the facts. It was based on the supposition that conviction may be a rational construct, the result of a certain compounding of cognitive factors which produce it and without which it would not appear. That supposition, however, is not a necessary one. Conviction could be, instead, a factor in itself which originates below the conscious level, just as do others of the various extrasensory elements which make up a psi experience.

In exploring the possibilities of this second supposition, it will be helpful first to review the outstanding characteristics of the low conviction and high conviction groups. Those which may be con-

sidered low are the Dramatic, the Symbolic, the Photographic, the Fragmented, and the Complete; also dreams in general and Precognitive experiences in general. The high conviction groups are the Blocked and the Meaningful, and the waking cases in general.

There is one characteristic which the low conviction groups have in common. Each of them calls for some degree of interpretation by the subject. They all represent cases in which the meaning is not self-evident. Dramatizations must be comprehended. Of course some are so simple as to be almost self-evident, but others are relatively complex. Symbolic experiences must be puzzled over. As reported, they often are not even thought to be meaningful until they have recurred one or more times. It will be recalled that conviction ratings for this group are very low. The Fragmented cases are also especially low, and these too are instances in which the essential meaning is not obvious. Instead the subject is confused or misled as to what the hidden meaning may be.

The Photographic group presents a confused situation because it consists of two kinds of cases. There are those that are incomplete as to meaning, and also those in which all elements of the essential meaning are present. One might expect incomplete cases to be believed less often than those in which the complete meaning is known, and therefore the fact that the conviction percentage of the Photographic group is not higher might be thought to be due to the presence of incomplete cases. However, the Complete group selected from the Photographic group shows that something other than incompleteness affects the conviction rate. The percentage of conviction in the Complete group, whether in the Undifferentiated or the Precognitive column of data in Table 4, is not appreciably different from that of the groups from which it was selected. Therefore, the observed percentages in both the Photographic and Complete groups seems to be related somehow to the type of case, rather than to the amount of knowledge shown. If the nature of a Photographic case, even a complete one, be considered, then it will be evident that it calls for a certain amount of interpretation, even if all elements of information are present. If the subject "sees" a wreck, the curve in the road, the trees by the wayside, the overturned car and his father's form motionless on the ground, the interpretation that father is killed must still be made. However

instantaneous, the meaning must be assembled and extricated from concomitant details. And therefore even Complete cases call for secondary mental processes in comprehending the data.

It would seem then that the conviction percentages in these different groups may be related to the amount of interpretation which each manner of presentation of the psi data into consciousness necessitates.

The association of dreams with low per cent of conviction as well as the fact that precognitive experiences tend to be less convincing than contemporary ones could also be explained in part on the same theory. Rational judgment would discount dreams more readily than waking experiences; and in a culture that knows nothing of precognitive mental faculties it would be especially difficult to accept knowledge of events which had not yet happened. However, an interpretation requiring secondary mental action could explain only the lowered conviction rates in the above groups, but it could not explain the fair percentage of dreams and precognitive experiences that are believed and it certainly could not explain the Blocked cases with their high percentage of conviction.

Turning now to the high conviction groups, Blocked and Meaningful, it is possible to see more similarity between them than at first appears. A special word about Blocked cases is necessary. It must be remembered that selection may play a larger part in cases of this kind than it does in other types, for these are based largely on emotional or compulsive experiences rather than on facts. Certainly only the most impressive cases of this kind would be accepted as possible instances of psi. It may be, too, that high conviction experiences of this type are more likely to be reported than less convincing ones. To whatever extent these selective factors may affect the case, still the frequency of conviction in the Blocked group, the rather special urgency of it as reported, and also the comparatively large number of cases that are reported, all seem to suggest that selection alone does not adequately explain the high conviction rate in cases of this kind. As mentioned earlier, these cases suggest repression, whether for protective or other reason. It seems as if the significant meaning fails to get into consciousness, but the emotion bursts through. Strong conviction appears also. The force

of the emotion and the unreasoning nature of the conviction each gives the impression of irrational, subconscious action.

Meaningful cases at first appear to be quite different from Blocked ones, for they are marked by knowledge of the meaning and by the absence of the excessive tension of the Blocked case, which may be the cause of the outbreak of emotion or compulsion to action. However, since Meaningful cases are largely stripped of detail, it is possible to think of them as cases in which some blocking effect may also have occurred. Possibly they are examples of blocking in which instead of emotion the essential meaning gets through, even though supporting details are unable to emerge. If so, then the blocking process could be considered a continuum, of which Blocked cases represent one extreme, Meaningful ones the other.

Possibly all Fragmented type cases would also result from blocking of some kind. But if so, they would hardly fall on the direct line between the Blocked and the Meaningful groups as far as the contrast here considered is concerned. As said before, Fragmentation cases require interpretation which Meaningful cases do not, and it may well be that the blocking process may work in various ways, only hints of which are given in a study as exploratory as the present one. At least, the tendency shown by the present data is for conviction for some reason to accompany the Blocked and Meaningful type case more frequently than it does any of the other types.

The high conviction groups of cases then seem to contribute some support to the suggestion that conviction is an element of experience in itself, originating below the conscious level. As such, it might or might not get across. While it may have some special linkage with emotion in Blocked cases and with significance in Meaningful ones, it does occur in a minority of cases in the low conviction groups.

Some hint as to the nature of the factor which governs such emergence or repression may possibly lie in obscure personality factors. For example, in those personalities in which for some reason involved symbolism is the necessary vehicle of transmission of psi impressions from the unconscious to the conscious level of

the mind, the same reason might make it difficult for the conviction element to emerge. Therefore, the occurrence of conviction too would be affected according to whatever the threshold situation was, and different personalities and different patterns of emergence would determine it.

The suggestion, then, that conviction is a distinct element of a psi experience originating below the conscious level would explain the fact that when it does emerge it is not easily affected by rational analysis. When it does not emerge reasoning would not be likely to produce it. Perhaps if it were so produced it would not achieve quite the same order of intensity that it does as a subconscious factor. Certainly reasoning would produce doubt rather than certainty, especially in connection with dreams and precognitive experiences.

This case survey has shown first that conviction occurs with considerable frequency in spontaneous psi experiences. It is reported from widely different sources and, although in varying proportions, in all types of cases. It is therefore a natural phenomenon to be studied by appropriate research methods and should no longer be overlooked by parapsychological experimenters.

Certain case classifications show sufficiently different ratios of conviction to non-conviction to yield important leads as to the nature of conviction. Perhaps the principal one is the suggestion that it arises out of non-rational, unconscious factors which may have something to do with the meaning and emotional quality of the experience. The suggestion too that blocking or repression of some type or types operates in the psi experience may be a profitable one for later investigation, if the suggestion has validity.

The different methods of expression of psi, the Dramatic, the Symbolic, and the Photographic, and also the different degrees of knowledge shown in Blocked, Fragmented, Meaningful, and Complete cases, raise questions concerning the operation and control of the threshold of consciousness that will need to be studied in relationship to conviction. What determines these limitations? Why does the threshold permit the passage of conviction in one case, inhibit it in another? Or why is the essential meaning transmitted in some cases, repressed in others? Are these limitations based on

personality characteristics? If so, what kind? And what control can the subject or the experimenter exercise when these questions are answered?

Such questions are only a few of the more self-evident ones raised by this analysis. If they prove stimulating or helpful to those planning research, then the method of drawing upon spontaneous cases for new research suggestions will have proved to be profitable.

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# ESP PERFORMANCE AND TARGET SEQUENCE

By S. G. Soal and J. G. Pratt

**ABSTRACT: This paper deals with a series of 33,500 ESP trials which were made from 1946-1950 by Mrs. Gloria Stewart, the subject in experiments carried out by Dr. S. G. Soal. These were data in which Mrs. Stewart had scored at a significantly high rate of success in calling five animal names which were designated as targets. Where 20 per cent of the trials are expected to be hits by chance, she averaged 25.9 per cent for the series as a whole.**

**The purpose of analyzing these data was to determine whether there was any relationship between success in hitting a specified target and the sequence or pattern of targets in which the one that was hit occurred. The authors have found highly significant relationships, two of which are presented here.**

**In making the analyses the targets were first divided arbitrarily into pairs. The pairs were then separated into two classes: those in which the two targets were unlike (non-doubles) and those in which they were alike (doubles), and the rates of scoring on the first member of the pair for the two classes were compared. The calls for the first member of target non-doubles gave hits in 26.8 per cent of the cases, while the calls on the first of target doubles scored only 17.2 per cent hits. The difference in these two scoring rates is highly significant.**

**The scoring rate was analyzed further in relation to the number of identical targets occurring consecutively in the target sequence (singles, doubles, triples, etc.). The highest rate of success was found on singles (single targets which were not preceded or followed by a target of the same denomination). As the number of identical targets in the sequence increased, the rate of scoring on the first target decreased.**

**The findings are interpreted as showing that Mrs. Stewart was responding with an ESP perspective that was not limited to a single target, and her work is compared with that of other subjects who showed similar effects. There are, however, some aspects of Mrs. Stewart's ESP performance that can be explained only in terms of her own personal motivation in the tests. Some hypothetical suggestions are offered regarding the motivating factors which may have produced the observed ESP effects.—Ed.**

## Introduction

During the five-year period from 1946 to 1950, S.G.S. in collaboration with Mr. F. Bateman carried out in England a series of ESP tests with Mrs. Gloria Stewart acting as subject (**1, 6, 7**). The present report is the first in a new series of reports of research dealing with these records[1]—research which was initiated during S.G.S.'s visit to the Parapsychology Laboratory in April and May, 1951, as a Fulbright Exchange Scholar. J.G.P.'s participation in this work was made possible through the financial support of The Rockefeller Foundation.

[1] We wish to express our appreciation to the members of the Parapsychology Laboratory, especially to Mr. Kenneth Bates and Miss Marietta Newton, for their assistance in this work.

The total amount of Mrs. Stewart's work approached 50,000 individual trials, most of which were based upon the use of target material that limited the subject to one choice among five possibilities. Most of the tests were carried out in Mrs. Stewart's home. When the participants had assembled and were ready to begin, Mrs. Stewart was seated at a table in one room with a pencil and a blank record sheet before her. At least one experimenter was assigned to observe the subject throughout the test to see that she did not leave her chair and to observe that she did not get out of step in making her record.

In another room out of sight of the subject and her observer was a table on top of which rested a three-foot square screen. The only opening provided by this screen was a three-inch square window in its center. The experimenter, generally S.G.S., sat at this table on the side of the screen nearer the subject, and someone assigned to act as sender sat on the opposite side. The experimenter had a set of five cards, numbered plainly from 1 to 5.

The sender had before him a set of five cards, each one bearing on its face one of the five objects designated as the targets for the tests. At the start of the series, the targets were pictures of five familiar animals, but later they were simply the initials of the animal names. The subject always recorded her responses in terms of the animal initials. The set of five cards was in a small box which lay on its side with the opening toward the sender. Thus he alone had a view of these cards except when special arrangements were made to have two or more senders work in conjunction (7).

Before the start of a sheet of 50 trials, the sender would shuffle his set of five target cards and arrange them face down for use under whatever conditions were specified for that particular test. Most of the tests were done under conditions that required the sender to look at the order of the target cards after they had been shuffled, and the subject was successful only under these conditions. It was not necessary, however, for the sender to look at the card selected as the target while the subject was writing down her response to it in the next room. Early in the series it was discovered that the subject did quite as well if the sender simply gazed for a few seconds at the new order of the targets on the bottom of the box and then turned them face down for the series of 50 trials.

A large stock of the digits from 1 to 5 were prepared two years in advance of the start of the experimental series by an assistant who offered his services for this task, and S.G.S. generally drew upon this stock of numbers to meet the requirements of the tests as they were planned for each weekly or biweekly session. The instructions to the compiler of the digits were to take the last digit of Chamber's seven-place logarithms at intervals of 100, ignoring cases in which the digit did not fall between 1 and 5. In advance of each experimental session, S.G.S. recopied these digits in order as they were needed, using 50 numbers to the sheet. These were written on record forms that provided spaces for two columns of 25 trials each.

When the sender and the subject were ready, the test proceeded as follows: The experimenter at the screen opposite the sender looked at the first digit on the sheet of numbers selected for that occasion and held up the card with that digit at the small window so that it was visible to the sender. The list of digits, which had been in the possession of the experimenter until this time, had been carefully safeguarded against inspection by anyone else. Even during the course of the 50 trials, it was visible only to the experimenter and occasionally to an observer who might have come to witness the experiments. No one was allowed to see both the list of digits and the order of the sender's targets during a set of 50 trials. As soon as the sender saw the digit at the window, he indicated the corresponding target card, counting from left to right, in the manner specified by the conditions of the test, generally by simply touching its back with his finger. After a pause of about a second to allow the sender time for this action, the experimenter signalled to the subject by calling "One" (or whatever the number of the trial was). This procedure was repeated for each of the remaining trials of the first column. Then, after a pause for a few seconds and without any rearrangement of the target cards, the whole procedure was gone through again for the second column.

At the completion of each sheet of 50 trials, the experimenter came around the screen and copied down the sender's order of the target cards as a code for interpreting the digits of that sheet. This was carefully witnessed and checked by the sender and any other observers present in that room, but not by the subject and her observers.

The experiment then continued for the next sheet, and so on through as many sheets as the plan of the session required. No checking of the results was done until all the work of the session had been completed, and then the subject did not take any part in the checking. Only rarely did she see any of the target records, and as a rule she was only told in general terms what her performance had been: "Better than usual," "Good," "Not so good as usual," "A poor showing tonight," etc.

The entire series of experiments contained 130 separate sessions. These were done with a wide variety of test conditions designed to throw light upon the manner in which different physical and psychological conditions affected Mrs. Stewart's ESP success. In general, she averaged approximately seven hits per column under those conditions that were favorable to high scoring, maintaining a level of success of about 26 per cent as compared with the 20 per cent rate expected from chance. Mrs. Stewart's success in this series came mainly in the form of hits on the direct target, the one designated by the sender at the exact time when she made her call. The results with the different test conditions are shown largely by the comparison of the total scores obtained in the different situations. Some reports on the findings from this point of view have already been made (**1, 6, 7**), but most of the results are still awaiting publication.

This is the first of a series of reports on Mrs. Stewart's work that will be concerned with how her ESP success was affected by internal or incidental factors—conditions that are defined in terms of trial-to-trial variations in the tests. These results were largely independent of the specific purposes and detailed plans of the investigators. Instead, they point to characteristics of ESP performance that cut across the many different favorable experimental conditions and that are, to a considerable extent, independent of them. In studying Mrs. Stewart's scoring in relation to the finer structure of the tests we hope, of course, to discover facts about how ESP operates that will be generally valid. We recognize, however, that many of the significant effects to be reported may be personal peculiarities of this individual subject. Even if in return for a great deal of work we find that we have gained relatively little general knowledge about ESP, the research still seems worth while.

At the present stage of parapsychological knowledge, research workers cannot afford to neglect any area of investigation that may advance our understanding of and control over the ESP process.

## Data

For these analyses we used as many of the trials from the 130 sessions as were suitable for the study of internal, trial-to-trial variations in conditions which might affect Mrs. Stewart's ESP performance. The following data were excluded from these analyses: (1) All clairvoyance tests, in which the sender shuffled his target cards and arranged them face down without looking at their faces until the 50 trials were completed, were left out, as they consistently gave only a chance rate of scoring. (2) Tests for "pure telepathy" in which there was no written record of the target order were necessarily omitted. (3) Trials which were performed at a rapid rate of calling were omitted, as these showed a shifting of scoring success from the direct target to the immediately preceding target (backward displacement). (4) Pages done with the subject's choices restricted to two possibilities were omitted from the main analysis, though these were used for a special study of target sequence to be described in a later report. (5) Likewise, tests with playing cards were excluded. (6) Experiments with two or three senders in opposition (i.e., concentrating upon different targets during each trial) were left out, since the results with the different senders were not statistically independent. (7) Eight sheets of a special experiment involving non-synchronized tests were omitted from sessions 56 and 57. (8) Finally, the last ten sessions were excluded as a block since during this time the subject's average scoring rate had declined to the level of chance expectation, and these results presumably would have served only to dilute the ESP effects in the high-scoring sections of the first 120 sessions. The omitted records are, of course, available for separate analyses to show whether the special conditions involved in them produced any trial-to-trial variations in ESP performance.

The data that remain for analysis after the above exclusions comprise 670 pages of 50 trials each, a total of 33,500 trials. These give a total of 8,691 hits as compared with an expected number of 6,700. This is an overall scoring rate of 25.9 per cent as compared with the expected 20 per cent rate. This is a highly significant

result, and the whole of Mrs. Stewart's records, pooling favorable and unfavorable conditions indiscriminately for the entire 130 sessions, yield a total score that is significant with a probability value of less than $10^{-50}$. The main part of Mrs. Stewart's succcessful ESP performance is contained in the 670 pages of data included in the present analyses. These records with a high level of ESP performance present an opportunity to see what, if any, factors within the finer structure of the test affected her rate of scoring. The selection of the higher-scoring records does not in any way affect the validity of the findings, as these depend upon comparing rates of scoring under different conditions within the data.

An examination of the target sequence revealed a nonrandom feature which necessitated a careful consideration of the validity of our application of the statistical methods we intended to employ. The evidence indicates that the existence of the peculiarity we found in the target series cannot affect the general conclusions we have reached in this paper. The basis of this conclusion is given in the detailed discussion of the distribution which is to be found in the appendix and in other checks we made in the body of the paper.

## Problem and Method of Analysis

Is the subject's rate of success in responding to an ESP target affected in any significant way by the pattern or sequence of targets in which it occurs? This is the general question with which this report deals. Specifically, we have been concerned with comparing the probabilities of making a hit when the target symbol the subject is trying to call is followed by the same symbol (that is, when it is the first of a target double) and when the target is followed by a different symbol (that is, when it is the first of a non-double).

The method of analysis first to be applied was as follows: Each column of the record sheet was considered as twelve non-overlapping pairs of trials. This left the twenty-fifth trial of each column, which was disregarded completely in this analysis. Each pair was then tabulated in terms of whether or not the two targets formed a double (that is, both targets were the same) or a non-double. Also, the first trial of each pair was tabulated in terms of whether the call scored a hit or a miss. This tabulation led to a division of the data into four categories, hits and misses on target doubles and the same for non-doubles. These figures lent themselves to a chi-square test

of whether the rate of scoring is the same on the first trials of target doubles and of target non-doubles. If the factors affecting ESP performance are the same in these two situations, we would expect to find approximately equal proportions of hits. Instead, a marked difference was observed and the results, discussed in the following section, were found to be highly significant.

The first general finding led to other analyses which were made with the object of throwing further light upon the observed target sequence effect. The most important of these was an analysis of *all* the trials of the 670 record pages in terms of the following target categories: singles, isolated doubles, isolated triples, isolated quadruples, isolated quintuples, and isolated sextuples.[2] (No instance was found of an unbroken sequence of more than six identical target symbols.) This analysis made it possible to see more clearly what kind of effect it was that had been revealed by the first general analysis described in the preceding paragraph.

Other analyses made follow in principle the two that have been explained. These can more conveniently be described in connection with the specific questions upon which they bear when these arise later in the report.

## Results

As there were 670 pages of data, each with two columns, and as each column provided 12 non-overlapping pairs of targets for the basic analysis, we examined a total of 16,080 target pairs. Of these, 3,270 were found to be doubles and 12,810 were non-doubles. (These sequences are not specified as regards their relation to the adjacent symbols in adjoining pairs.) This is in close agreement with the expected number of 3,216 doubles and 12,864 non-doubles, respectively. When the proportion of hits on the first targets of these two classes of pairs are compared, a striking difference in scoring rate is found. On the first of doubles, only 17.2 per cent of the calls were hits, while on the first of non-doubles, 26.8 per cent were hits. The complete results of this analysis are shown in Table 1. The upper figure in each cell of the table is the observed number of cases, and the expected number is given in parentheses.

[2] By a "single" is meant any symbol which is not a member of a double or longer sequence of like symbols. Similarly an "isolated double" is any double that is not a part of a triple or longer sequence, and so on. In this analysis, each column was treated as a separate unit, no carry-over being made from the end of one column to the beginning of the next one.

Table 1

RATE OF SCORING ON THE FIRST MEMBERS OF NON-OVERLAPPING TARGET PAIRS

| | Target Pairs: Doubles | Target Pairs: Non-Doubles | Total |
|---|---|---|---|
| Hits on First Trial | 563 (812.2) | 3431 (3181.8) | 3994 |
| Misses on First Trial | 2707 (2457.8) | 9379 (9628.2) | 12086 |
| Total | 3270 | 12810 | 16080 |

$\chi^2 = 127.71$ (1 d.f.)
$P = 10^{-27}$

The chi-square value of 127.71 is highly significant and leaves no room for doubting that Mrs. Stewart showed a higher rate of scoring on the first of non-doubles than on the first of doubles. The score on the first of doubles is not simply a failure to demonstrate ESP. It is significantly below expectation (CR = 3.98; P = .00007).

The question arises regarding how this difference in scoring rate is distributed over the entire four-year period of ESP testing. Was it relatively constant over this entire time, or did it show significant changes related to the amount of the subject's experience with the tests? The data were divided into eight chronological samples of approximately equal size. In general, it was found that the results in each sample agreed closely with the trend shown by the analysis of the total data. The essential details of this chronological study are shown in Table 2, where the rates of scoring on the first of doubles and of non-doubles are shown in terms of percentages of hits to total pairs in each category, and where the chi square for each sample is also shown. Every one of the eight samples gave a significant value of chi square.

As explained in the appendix, there was a significant deficit of instances in which a particular target is repeated in the second following position, as ABA. This deficit of (ABA) patterns in the target sequence suggested the need for careful consideration of possible statistical artifacts before interpreting the difference in

Table 2

CHRONOLOGICAL DISTRIBUTION WITHIN EXPERIMENTAL SERIES OF SCORING EFFECTS ON THE FIRST OF TARGET DOUBLES AND NON-DOUBLES

| Sample Number | Data Sheets | Target Doubles | | | Target Non-Doubles | | | Chi Square |
|---|---|---|---|---|---|---|---|---|
| | | Pairs | Hits on First | Per-cent | Pairs | Hits on First | Per-cent | |
| 1..... | 88 | 406 | 77 | 19.0 | 1706 | 461 | 27.0 | 11.21 |
| 2..... | 76 | 382 | 68 | 17.8 | 1442 | 449 | 31.1 | 26.45 |
| 3..... | 76 | 391 | 62 | 15.9 | 1433 | 383 | 26.7 | 19.68 |
| 4..... | 85 | 416 | 65 | 15.6 | 1624 | 411 | 25.3 | 17.36 |
| 5..... | 85 | 411 | 63 | 15.3 | 1629 | 431 | 26.5 | 22.15 |
| 6..... | 85 | 401 | 67 | 16.7 | 1639 | 420 | 25.6 | 14.10 |
| 7..... | 85 | 426 | 76 | 17.8 | 1614 | 385 | 23.9 | 6.97 |
| 8..... | 90 | 437 | 85 | 19.5 | 1723 | 491 | 28.5 | 14.59 |

success on the first of doubles and non-doubles as an ESP effect. What was required was some control situation offered by the data themselves which would show that the peculiarities of the target series do not produce this effect if the usual psychological factors were not present. What special case did the data offer in which the difference in scoring on the first trial of doubles and non-doubles should be predicted to occur if it was purely a statistical artifact, but would not be predicted on the ESP hypothesis?

The control study needed was one that would duplicate all the conditions of the experimental series except the psychological factors involved in the subject's ESP performance. This purpose was achieved by assuming that the hits made by Mrs. Stewart on one column of targets were actually made in the same positions on another designated column. This kind of control analysis, which was suggested by Mrs. Esther Foster, has been carried out on a sufficiently large scale to get enough data to compare with the experimental results. Specifically, the procedure used in the control study was as follows: When Mrs. Stewart actually scored a hit on a particular trial of column (*a*), the first one on the page, she was credited with an *assumed* hit on the corresponding trial of column (*b*) *if the same target appeared at this point in the two columns.* The reason for counting a control hit in column (*b*) only if the target was the same as the one on which the subject scored a hit at that point in column (*a*) was to take account of any possible tend-

ency for the sequence effects to occur in association with some targets more than with others. The control hits obtained in this manner formed a series with all the characteristics of Mrs. S.'s ESP data (the same level of scoring, position effects, possible target preference, and target sequences as in the experimental series). Statistically, therefore, the control series and the experimental series appeared to be equivalent, and we should expect to find the same sequence effects in the two if they are produced as any kind of statistical artifact.

To test the matter, the "control" hits in the 670 (*b*) columns were observed to see whether they occurred on the first of a target double or non-double, and the ratio obtained was compared with the corresponding figures from the experimental series, as shown in the upper two cells of Table 1. In the control series, 18.0 per cent of the hits occurred on the first member of target doubles, whereas in the experimental series only 14.1 per cent of the hits were in this category. The chi-square test of the two sets of data (Table 3) shows that the difference is significant (P = .005). We have used only a small part of the data available in this control study, but as it is clearly significant on the present scale there seems to be nothing to be gained from extending the size of the sample. The result obtained shows that Mrs. Stewart's pattern of high scoring when artificially coupled with the actual target order does not produce sequence effects like those found in her experimental results. The explanation of the latter must therefore be sought in the psychological factors present in the actual ESP test situation.

Table 3

SPECIAL CONTROL STUDY OF TARGET SEQUENCE EFFECTS
(See Text)

| | Target Pairs: Doubles | Target Pairs: Non-Doubles | |
|---|---|---|---|
| Control Hits | 143 (117.2) | 652 (677.8) | 795 |
| Experimental Hits | 563 (588.8) | 3431 (3405.2) | 3994 |
| | 706 | 4083 | 4789 |

$\chi^2 = 7.99$ (1 d.f.)
P = .005

For further evidence supporting this conclusion, it is interesting to see the results for the first member of doubles and of non-doubles for the first pair of the (*a*) column. The first trial on the sheet is one that may be said to be the most nearly free of peculiarities both in the target sequence and in the subject's call sequence. The sender's shuffling of the target cards provides a new code at the start of each page for the conversion of the digits to animal targets. Also, the first trial on the page represents a new start for the subject and that call therefore appears to be the one that is least likely to be affected by the preceding call. In arbitrarily selecting *one* target pair from each page as a sample that would be free of the target order peculiarities found only *within* the sequence of 50 targets of each page, we naturally selected the first pair on each page.

The results of the analysis of the first pair of the (*a*) column for the target doubles effect gives a chi square of 12.59 with one degree of freedom (Table 4). This is highly significant, in spite of the fact that only one-twelfth of the total data are involved (P = .0004). Relatively speaking, the contrast between the rates of scoring on doubles and non-doubles in this sample is even more striking than for the data as a whole. The rate of scoring on the first of a double in this special pair is 14.6 per cent, and that on the first of a non-double 28.9 per cent, a difference of 14.3 per cent. For the total data of the 670 pages, the corresponding figures are 17.2 per cent and 26.8 per cent, a difference of 9.6 per cent.[3]

[3] Since the results found in the first pair of the (*a*) column are sufficiently significant to support the main conclusion reached in this report, we made a special study of the two distributions represented in Table 4 to answer the question whether the chi-square test was strictly applicable. The two distributions in question are: (1) the animal initials representing the 670 successive *first* targets on the page; and (2) the order of occurrence of *doubles* and *non-doubles* for the 670 first target pairs on the page.

(1) The distribution of the 670 *first targets* on the pages has been checked for randomness in the following manner: The target digits for these trials were decoded into the animal initials by means of the code at the foot of each sheet. In the total series of 120 sessions, there were three sets of animal initials: E, G, L, P, Z for sessions 1-42; C, F, H, R, T for session 43; and C, F, H, K, T for sessions 44-120. In order to reduce these to a uniform set of targets for the purpose of the present study, the letters A, B, C, D, E were considered as representing the three sets of animal initials, in alphabetical order as shown. To this uniform set of 670 "targets," the following tests of randomness were then applied. The number of occurrences of each of the five targets was observed. The frequencies of A, B, C, D, and E, respectively, were 140, 128, 142, 124, and 136, where 134 is the expected number. This distribution gives a chi-square value of 1.8 (4 d.f.) with P lying between .7 and .8. There were 426 singles (429.12 were expected), 85 isolated doubles (85.70 expected), 22 isolated triples (17.11 expected), 2 isolated quadruples (3.42 expected), and no longer sequences (as compared with 0.85

Table 4

TARGET SEQUENCE EFFECT IN THE FIRST PAIRS ON THE 670 RECORD SHEETS

| | Target Pairs: Doubles | Target Pairs: Non-Doubles | Total |
|---|---|---|---|
| Hits on First Trial | 22 (38.8) | 150 (133.2) | 172 |
| Misses on First Trial | 129 (112.2) | 369 (385.8) | 498 |
| Total | 151 | 519 | 670 |

$\chi^2 = 12.59$ (1 d.f.)
P = .0004

From the point of view of consistency, the trend for doubles to score low is present in nearly all the 24 pairs analyzed from *within* the columns. To cite one further example, the first pair of the (*b*) column, like that of the (*a*) column, is significant, the chi square being 8.90 (P = .003). This consistency of the findings where one expects them on psychological grounds regardless of how the data are sliced is further assurance, if it is needed, that the low score associated with doubles is a psychological effect and, under the general test conditions, one which can only be attributed to an as yet unexplained difference in ESP scoring.

After careful consideration of the matter of the deficiency of

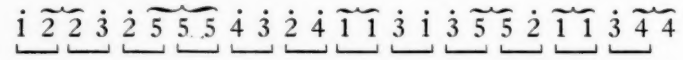
expected). Also, there were found 110 instances of the (ABA) type of pattern compared with the expected number of 106.88. These figures for the various types of target situations studied in this investigation show in every instance a close agreement between the observed and the expected numbers for this special sample of 670 targets.

(2) The distribution of doubles and non-doubles for the 670 pairs was checked in the following manner. The order of occurrence of doubles (D's) and non-doubles (N's) for the chronological order of the series was listed. Two statistical tests that had been decided upon in advance were then applied. (*a*) The distribution was divided into five equal groups of 134 pairs each, and a chi-square test was applied to measure the uniformity of occurrence of D's in the different sections of the series. The value of chi square found was 6.42 (4 d. f.), with P = .09. (*b*) Steven's test of sequences (*Ann. Eugen.*, 1939, **9**, 10-17) was applied to see how closely the number of groups or loci in which the D's occurred conformed to the number of groups expected in a random ordering of the observed number of D's and N's in a single row. The D's were found to occur in 111 groups, where 118.2 groups were expected. This represents a deviation which is 1.6 times the standard deviation (P = .1). The distribution of doubles and non-doubles in the 670 pairs is therefore sufficiently close to theoretical randomness to justify the use of chi square and to support the conclusion that the low probability shown in Table 4 can only be interpreted as an adverse effect of doubles in the targets upon ESP success.

(ABA) patterns in the target sequence, we are certain that it does not affect the findings in any serious way and that the conclusions to be drawn are the same as they would have been if this question had never been raised. In the further study of Mrs. Stewart's ESP series, we shall discuss the target sequence peculiarities only if the question is relevant to the particular analysis or aspect of her performance under consideration at the moment.

After an ESP effect related to doubles in the targets had been established by the foregoing general analysis in terms of non-overlapping pairs, we proceeded to a more detailed study of this effect in the effort to throw some light upon its nature. The results of only one such analysis that has been completed will be reported here, and the results of others will be given in later reports.

In this analysis, the entire series of 33,500 trials were tabulated in terms of target digit groups. As described earlier, the categories observed were: singles, isolated doubles, isolated triples, isolated quadruples, etc., up to sextuples, the longest sequence found. For example, we may consider a sequence of 25 digits such as the following, corresponding to a typical target order for one column.



In this example there are 12 singles (indicated by the dots), five isolated doubles, and one isolated triple. In the earlier, basic analysis of the target sequence effects, this sequence of 25 targets would have been divided into the 12 pairs indicated by the brackets shown below the row of digits. It may be seen that the previous method of breaking the digits up into non-overlapping pairs yields three doubles and nine non-doubles. Close inspection will show that six of the non-double pairs actually contain either a first or second member that belonged to a sequence of two or more of the same digit, the member being split off by the arbitrary division into non-overlapping pairs.

The method of taking singles, doubles, triples, etc., without breaking up any actual sequences should be more sensitive than the more arbitrary procedure previously applied. While the former analysis was adequate for the purpose of demonstrating the effect

of target sequences upon ESP performance, this one obviously should be more informative for revealing variations in the effect and therefore more profitable at more advanced stages of the investigation. Futhermore, there is no statistical objection to this method of analysis as long as we confine our attention to the rate of scoring on the singles as compared with the rate on the *first trial* of the longer sequences. These comparisons are based upon data in which equal ratios of success would be expected on a basis of random sampling.

Considering for the moment only the singles and the first trials of longer sequences, the results show that there is a fairly regular decline in scoring as the size of the group of identical targets increases. Hits were scored on 29.4 per cent of the singles, on 18.8 per cent of the first member of target doubles, and on 13.2 per cent of the first member of triples. In general, there was no further decline in rate on the first member of quadruples and longer sequences. The percentage of success for the first trials of these longer sequences combined is 15.9, slightly higher than that for triples.

The scoring rate on singles was compared by means of a chi-square test with the first trial of isolated doubles. As mentioned above, the rate of scoring on the former was 29.4 per cent, while that on the latter was only 18.8 per cent. A chi-square test of the actual data upon which this comparison was based proved to be more significant than any yet evaluated. This is because the effects being tested are present in a more nearly "pure state" in this analysis and also because this method of tabulation yields a larger number of observations than that of taking non-overlapping pairs. The results are shown in Table 5.

To answer the question whether the lowering of the scoring rate is proportional to the length of the sequence of identical targets, we made a similar chi-square test of the data for the first member of isolated doubles as compared with the first member of longer sequences. The proportion of hits on the first trial of the longer sequences was found to be significantly less than that on the first trial of the isolated doubles (see Table 6). Since this analysis was planned on a purely logical basis before the analysis in terms of target digit groups was begun, it does not represent a favorable

Table 5

EVALUATION OF THE DIFFERENCE IN SUCCESS ON SINGLES AND THE FIRST MEMBER OF ISOLATED DOUBLES

| | Target Category: Singletons | Doubles (1st member) | Total |
|---|---|---|---|
| Hits | 6377 (5989.1) | 821 (1208.9) | 7198 |
| Misses | 15278 (15665.9) | 3550 (3162.1) | 18828 |
| Total | 21655 | 4371 | 26026 |

$\chi^2 = 206.76$ (1 d.f.)
$P = 10^{-44}$

grouping of the data based upon an inspection of the possible ways of arranging the results. Rather, the analysis was a straightforward test of the logical inference that isolated doubles should show a higher scoring rate than longer sequences, and the result is a clear-cut verification of that hypothesis.

The rate of success on the later members of target sequences is not independent of that on the first member, as it is affected by nonrandomness in the subject's call sequence. It is interesting to observe, however, that the subject showed a general tendency to

Table 6

RATE OF SCORING ON THE FIRST MEMBER OF ISOLATED TARGET DOUBLES COMPARED WITH THE FIRST MEMBER OF LONGER TARGET SEQUENCES

| | Target Sequence: Isolated Doubles | Isolated Triples, Quadruples, etc. | Total |
|---|---|---|---|
| Hits on First Trial | 821 (780.4) | 132 (172.6) | 953 |
| Misses on First Trial | 3550 (3590.6) | 835 (794.4) | 4385 |
| Total | 4371 | 967 | 5338 |

$\chi^2 = 14.22$ (1 d.f.)
$P = .0002$

avoid calling doubles, averaging about 14 per cent for the series as a whole where 20 per cent would be expected. This deficiency of call doubles, coupled with the low score on the first member of like target sequences, should lead to a higher rate of scoring on the second trial, while a low rate on the second trial should produce a higher rate on the third, etc. Therefore the later members of like sequences should be in a more favorable situation than the singles, if they were equal to them as ESP targets in all other respects.

In spite of the slight degree of interdependence of scoring within a like-symbol sequence, it is worth noting that if we consider the groups as a whole we find a more regular relationship between decline in scoring and the length of the sequence than we find for the first members of the groups taken alone. Indeed, the group scoring rates for singles, doubles, triples, quadruples, quintuples, and sextuples are, respectively, 29.4 per cent, 20.9 per cent, 16.5 per cent, 13.8 per cent, 13.3 per cent, and 4.2 per cent, and these show a consistent decline.

## Discussion

In order to make any progress toward understanding the difference in rates of scoring on sequences of identical and unlike targets, it seems necessary to use the now familiar concept of ESP displacement. It is the *second* member of a target pair that distinguishes the first member of a target double from a target single. The difference in rates of scoring on the direct targets in these two cases indicates, therefore, that the subject was in some sense aware by ESP of the *following* targets and that her calls were influenced by this spread of perspective, or ESP displacement effect.[4]

[4] An interesting logical possibility of explaining the results without the necessity of supposing that the subject herself was responding *directly* to both the direct target and the one ahead has been suggested by Mrs. Esther Foster. The experimenter who is holding up the numbered cards at the screen window may see on his list not only the digit for a given trial, but those for the following trials as well. Now it is conceivable that the experimenter may be serving in a special way as a sender or agent for the subject in the test situation. In this role, he could not directly provide information about the actual animal targets, but he could have different attitudes toward individual trials depending upon whether he saw that the digit card he was holding up had to be used again or not. For example, seeing a sequence of the same digit on the list may have led the experimenter to feel: "Now I must watch my step or I will lose my place." If so, the subject might unconsciously get by ESP from the experimenter the impression that she could not succeed on these trials. This might produce a change in the subtle motivation involved in her ESP responses to the animal targets themselves

The analyses that have been reported in the present investigations do not form an exhaustive study of the possibilities for displacement in Mrs. Stewart's work. They do show, however, that the rate of direct target successes is affected by whether or not the call that would score a direct hit would at the same time make a hit on forward or backward displacement. The subject's tendency was to make her direct hits on targets which would not allow her to score a forward or backward displacement hit, and to avoid direct hits otherwise.

Fairly extensive investigations of this subject's records have been made for displacement effects in areas of the column where no direct target hits occurred. The results indicate that there is no general displacement in her work. A more detailed report on this series analyzed from the point of view of general displacement will be made later.

The findings of the present investigation are related to some of the displacement results that have been reported for the two outstanding subjects of the Martin and Stribic series (**3, 4**). Just as those two subjects differed from each other in their performance, we find that Mrs. Stewart's results also show individual peculiarities. All three subjects agree in showing a lower rate of scoring on the first of a double in the target series than on the first of a non-double. In the case of the Colorado subjects, however, there was a rise in scoring on the second of a double and the difference between the two members of a target double was statistically significant. In the case of Mrs. Stewart, there is only a slight rise on the second of a double (except in the case of the first fifteen sittings, where the second of target doubles showed an unusually high level of scoring). In general, we may say that the lower number of direct hits found on the first of a target sequence seems to be characteristic in her work of other members of the sequence to an almost equal degree.

The preceding number of the JOURNAL carried a report of the

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and cause her scoring success to decline. This hypothesis would account for the results without supposing that the subject herself was showing displacement in the usual sense. It does not eliminate the use of a widened perspective in her ESP response, however, as it only substitutes a "perspective of senders" for the "perspective of targets" otherwise required. This follows from the fact that the score on doubles did not simply drop to the chance level but to a level significantly *below* expectation.

reinforcement effect (2) which was based chiefly on the work of B.S., another high-scoring subject investigated by S.G.S. It will be recalled that in some of his work, B.S. showed a tendency to score positively both on the preceding target (backward displacement) and the following target (forward displacement). In these particular tests, the rate of scoring was significantly higher when his call happened to fall between two identical targets than when the preceding and following targets were different. From the statistical point of view, there is a striking parallel between the effects discovered in Mrs. Stewart's work and those reported previously for B.S. With Mrs. Stewart, as with B.S., we find a significant difference in scoring rate when the call comes opposite two or more like symbols as compared with when it comes opposite unlike targets. The parallel breaks down in one important respect. In the case of Mrs. Stewart, unlike that of B.S., the pattern of identical targets produces a significantly *lower* rate of scoring.

In spite of the differences in detail, these two series do support each other in pointing to an important characteristic of the ESP process. The findings show that ESP is a process occurring with a perspective which is not limited to the individual target. In spite of the fact that the main evidence for ESP in these two subjects took quite different forms, the results seem to show that they were both responding, not to single targets, but to target *sequences* or *patterns*.

A widening of ESP perspective may be thought of as an extension of the scope of effective psi action in terms of either spatial or temporal aspects of the target situations. Since the effects under investigation were studied in relation to patterns of targets on the record sheet, it is easy to fall into the habit of thinking of the *spatial* features of the test as the more important ones. This implies, however, that the subjects were making their ESP responses directly to the physical targets, that is, that they were demonstrating powers of clairvoyance. Neither one of these subjects seemed to be able to score significantly in deliberately designed clairvoyance tests. Probably, therefore, their performance was being influenced by two or more events following one another in time. It is likely, in other words, that they were showing a "time perspective" in their ESP scoring. The evidence points strongly to the successive acts of the

sender in designating the target cards as an essential aspect of this time sequence. Whether this is a necessary interpretation is not relevant to the present discussion. Here we wish merely to point out that the most direct interpretation of the findings is that the subject was influenced in making some of her calls by events that were to take place a moment later in time, and possibly by events that had occurred a moment previously as well. This evidence in itself would not suffice to establish precognition; but as there is so much direct evidence that precognition occurs, the present findings should be kept in mind as suggesting that time displacement may be a quite common feature of ESP performance.

The evidence shows, then, that ESP at times is not simply a matter of making an occasional hit upon an isolated target. It is important to see to what extent this discovery can be confirmed and extended. Is the current emphasis upon perspective and configuration in the study of sensory perception, particularly the findings of Gestalt psychology, appropriate for extrasensory perception?

There are two approaches open for further investigations bearing on this question. One is that of studying the accumulated ESP data of still other subjects in the hope of throwing further light upon the present findings. The other is the planning of new experiments to test the hypotheses that have emerged. The former method has certain advantages, inasmuch as the records are already available and are even abundant enough to allow some choice in the comparison of experimental conditions. There are, however, limitations imposed by this analytical, survey type of investigation, and it will sooner or later become less rewarding to the investigator than new experiments designed and conducted with specific problems in mind.

Many aspects of Mrs. Stewart's performance are undoubtedly characteristic of her work alone and are the result of her own personal motivation in the tests. We can only suggest what the motivating factors may have been. Those who worked directly with Mrs. Stewart in these experiments got the impression that she thought of herself as competing with B.S.'s performance in his extensive experimental series done during the war. S.G.S. has suggested that since B.S. had been chiefly outstanding in scoring displacement hits, Mrs. Stewart may have wished to play a distinctive role by not showing any evidence of displacement. This may be

why she got her primary success in the form of hits on the direct target, whereas in 1936 she had chiefly scored forward and backward displacement hits (**5**). Also, it may explain the fact that she avoided getting direct target hits when the calls required to make those hits would at the same time give her forward or backward displacement. On this hypothesis we should have to suppose that she was showing in this series a limited kind of ESP displacement. We might speculate, for example, that her precognitive ability has now grown so weak that she is only able to recognize the target one or more steps ahead when it is reinforced, that is, when it is the second of a double or higher run of like cards. Whether this hypothesis is the correct one we cannot now say. What is needed is some way to test it, either in the data we now have or in new work with Mrs. Stewart.

It is interesting to try to relate the present findings to what has previously been known about how ESP operates. Results of the specific type covered in this report depend upon a kind of analysis that has only recently been applied to ESP records, and it is too soon yet to say to what extent they will be found in the data of other subjects. Attention has been called to the fact that similar results were obtained from S.G.S.'s earlier work with B.S. and from two subjects in the Martin and Stribic experiments. This is enough to show that we are not dealing with results that are wholly peculiar to Mrs. Stewart's ESP performance. These effects, however, are relatively weak in comparison with the statistical significance usually obtained from assessing the total score. For that reason, many series that were highly significant by ordinary standards would not provide enough data for this type of investigation.

Without attempting to make a complete list, we may call attention to a few aspects of the general evidence for ESP to which these findings are indirectly related. (1) The blind matching type of test, in which the subject must make a comparison by ESP of two "unknown" targets in order to score a genuine hit, involves the use of a perspective that is not limited to a single target. (2) Likewise, the DT test in which the subject responds not only to the target but also to the *position* of the card in the unbroken pack seems to involve a response to more than one aspect. (3) Distance tests in which the subject has never been at the place where the sender

or cards are located likewise involve what appears to be a complex response to more than one essential aspect of the situation. When, in addition, the sender is removed to a place other than the one at which the subject was told he would be, the situation is complicated to a still further degree. (4) Also, there are other results from Mrs. Stewart herself in which the function of the sender was split between two individuals. These tests made it necessary for the subject to put together information, part of which could only be obtained from one sender and part from the other. The fact that she got highly significant ESP scores under these conditions shows that she is capable of using a widened perspective of ESP to include two senders (see footnote on p. 207). ESP has been shown to operate under these and similar conditions that require a broadening of psi perspective to include more than a single target.

## APPENDIX

### Concerning the Target Lists

Certain tests for randomness were applied to the data of the target distribution. It was found that the numbers of the five animal initials E, G, L, P, Z employed in the first 42 sessions were closely equal. Further, the numbers of the initials C, F, H, K, T used in all but one of the remaining sessions considered in this paper approximated closely to equality.

Also the number of doubles was counted (three identical target symbols in succession being estimated as two doubles, four alike in a group as three doubles, etc.) and was found to be in close agreement with the expected number. Similarly the number of triples was counted (a quadruple being estimated as two triples, etc.) and was found to differ from the expected number by an amount within the limits of random sampling.

We have found, however, a very significant deficiency in patterns of the type (ABA) in which a pair of like symbols are separated by a different symbol. The assistant who prepared the stock of digits which determined the sequence of animal targets did this work about the year 1943 before the present series of experiments began. There is little doubt that this gentleman failed to carry out his instructions since the target lists of the Soal-Goldney series (8) which were selected personally by S.G.S. from seven-place logarithm

tables by the method described earlier in this paper, did not show the deficit of (ABA) patterns. Possibly the digits may have been selected at too close intervals. But the compiler had never met Mrs. Stewart or seen any of her records and he had no other connection with the experiments. It is now impossible to discover precisely what he did, since news has reached S.G.S. that he died a few years ago.

The important consideration now is to make reasonably sure that our statistical treatment of the data is valid and that we are not misled into wrong conclusions.[5]

Fortunately it is easy to demonstrate from the actual records that the deficiency in the pattern (ABA) had no effect on the expectation of direct hits on the target card. It will be recalled that each of the 670 sheets contains two columns of 25 trials each, known as the (*a*) and the (*b*) columns. We have made a complete cross-check by scoring for direct hits the targets of the first column on the page, the (*a*) column, against the calls of the second or (*b*) column, and the calls of column (*a*) against the targets of column (*b*). We have in all 33,500 trials and the cross-check registers 6,711 correct guesses as compared with a binomial expectation of 6,700. The standard deviation, assuming $p = \frac{1}{5}$, is 73.2. Moreover the observed standard deviation for 25 trials in the cross-check is found to be 1.962, which is in close agreement with 2.000, the binomial standard deviation.

Now Mrs. Stewart scored 8,691 hits on the 670 sheets, which gives an excess equivalent to 27.2 standard deviations. Had her guessing habits been correlated in any way with any nonrandom aspects of the distribution, the cross-check would assuredly have revealed a significant excess or deficit of direct hits, and this is not the case. Moreover, the totals for the cross-check were made after every ten sittings and at no stage in the count did the accumulated deviation of observed total from binomial expectation reach 1.5 standard deviations.

We have gone further and fitted the distribution of scores produced by the cross-check to a binomial distribution with $p = \frac{1}{5}$. That is, we have compared the expected numbers of scores of (0,

[5] The figures on the detailed analyses of the target sequence may be obtained by writing to the Parapsychology Laboratory.

1), 2, 3, 4, ....9, (10, 11, 12....) with the observed numbers and find $\chi^2 = 14.23$ with 9 d.f., giving P between .1 and .2, a result which is not abnormal.

Though the above analysis of the results from the cross-check provides ample justification for the use of binomial formulae in evaluating direct hits, there are additional considerations which may be mentioned. For instance, there are 2,600 trials in which the "telepathy" condition was excluded and these "clairvoyance" trials were rigidly alternated or randomly mixed in batches of 50 with an equal number (2,600) of trials in which telepathy could operate. But while the "clairvoyance" tests gave only 509 hits, a chance result, there were 707 hits on the "telepathy" trials. This very consistent difference cannot be explained in terms of any statistical artifact; it is clearly a psychological effect.

Again, in the London-Antwerp experiments the random numbers were on four occasions out of six compiled either by S.G.S. or by members of the mathematical staff of Queen Mary College working under his direct supervision. These four sittings show the same high rate of scoring as occurs in the remainder of the work. Again, a series of 1,600 trials in which the agent or sender was a person in Cambridge who was unacquainted with Mrs. Stewart produced only chance results, whereas a control series of 1,600 trials carried out on the same evenings, in which the agent was a person well known to the percipient and in the next room, gave an excess over expectation of 4.56 standard deviations.

Mrs. Stewart had only a passing interest in the raw data and only limited opportunities to observe the target digits. Needless to say, she never saw the lists of digits before an experiment. She was not told when she had made a hit but only learned of her general level of success at the end of the session after the experimenters had checked the results. Only on very rare occasions did the subject see the scoring sheets after the completion of the day's work.

We recognize the need to proceed with more caution than would be required if the target peculiarities did not exist, but we prefer to accept this responsibility rather than risk abandoning what may be important discoveries concerning the nature of ESP. The question of the value of effects such as those discussed in this report

must ultimately be answered in any case by their consequences as shown in future research.

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# BOOK REVIEWS

"Symposium: Is Psychical Research Relevant to Philosophy?" by Mrs. M. Kneale, Mr. R. Robinson, & Mr. C. W.K Mundle, from Aristotelian Society's Supplementary Volume XXIV, PSYCHICAL RESEARCH, ETHICS, AND LOGIC. London: Harrison and Sons, 1950.

Early in July of each year the Aristotelian Society and the Mind Association hold a Joint Session in one or another of the British universities. At each such Joint Session a number of symposia are held on certain prearranged topics, and to each such symposium three or four university teachers in philosophy contribute papers which are then collected and published by the Aristotelian Society in what is called a "Supplementary Volume." These volumes contain some of the most important contributions which British philosophers have made to contemporary philosophy.

In July, 1950, the Joint Session was held at the University of Bristol. It had last been held there in 1937. On that occasion for the first time one of the symposia dealt with a topic connected with psychical research, namely, "The Philosophical Implications of Foreknowledge." On the present occasion the organizers of the Joint Session decided that the return to Bristol would be a fitting opportunity for another symposium on a similar topic. This time they chose the question: "Is Psychical Research Relevant to Philosophy?" The present reviewer acted as Chairman at this symposium, and the remarks which follow are those which he made in opening the discussion. As will appear from them, he cannot honestly pretend to think that the symposium was a very valuable contribution to its subject. Mr. Mundle's paper was much the best of the three. Mrs. Kneale's was somewhat disappointing and Mr. Robinson's quite worthless.

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If I may say so with respect, I think that there are two things in Mrs. Kneale's paper which diminish its value as an introduction to the Symposium. One is that she thought it desirable to argue her case on the assumption that philosophy consists of linguistic

analysis. The other is that she has given an ostensive definition of "psychical research" by reference to Mr. Tyrrell's book, *The Personality of Man.* As regards the former, I can only say that I seldom find it illuminating to discuss philosophical questions in purely linguistic terms. It seems to me often to involve translating fairly straightforward and simply expressible questions into a contorted and pedantic terminology, with the risk of omitting factors which are relevant and introducing others which are contingent and irrelevant because they depend on the linguistic usages of particular races and cultures. As regards the latter, it is well to bear in mind that Mr. Tyrrell, beside having made valuable contributions to experimental and theoretical psychical research, is an enthusiastic amateur philosopher, and that he also strongly believes that a general recognition of the established results of psychical research would have far-reaching good effects. This latter personal conviction of Mr. Tyrrell's seems to have acted as a red herring to Mr. Robinson and led him to conclude his paper with a discussion of the effects for good or ill which would probably follow if telepathy, clairvoyance, and psychokinesis were not merely accepted as facts but became applicable in practice on a large scale. These speculations seem to me to be completely irrelevant to the subject of our Symposium. I doubt whether they are particularly relevant even to Mr. Tyrrell's enthusiasms; for what he was concerned with was the practical effects of the change in general opinion about the nature of man which he thought would follow on the general acceptance of the results of psychical research. It is obviously possible that the effects of that change of opinion might be predominantly good, while the effects of the widespread practical application of the powers by the general recognition of which the change had been brought about, might be predominantly bad.

Apart from the ostensive definition of "psychical research" by reference to Mr. Tyrrell's book, Mrs. Kneale makes two statements about the contents of that subject. One is that all the phenomena dealt with by it are "abnormal" in one or other of the following two ways. They are either (1) *physical* happenings of a sort not explicable in terms of the laws known to common sense or science, or (2) the acquiring by human beings of *information* which they could not obtain in normal ways. The other statement is that

psychical research is what is left over when the regular sciences have marked out their territories. Both these statements are negative; but the former at least tries to state a negative property common and peculiar to psi phenomena, while the latter merely delimits the subject by exclusion from the territories of other named subjects. The latter seems to me wholly unsatisfactory. The territories of the various sciences are not definitely fixed, and I should have thought that much that falls outside the present territories of all of them forms no part of the subject matter of psychical research. Let us therefore concentrate our attention on the former criterion, namely, that of "abnormality."

Mrs. Kneale does not develop this theme further; but Mr. Robinson makes some remarks about it, which I will now consider. Taking first the case of physical phenomena, he says that if the data of the psychical researchers and the laws accepted by the physicists were to conflict, the problem would be like the conflict between the theologians and the biologists in the nineteenth century. I cannot see the least resemblance between the two. What we should have is a conflict between the results of certain carefully conducted and controlled experiments (for example, on dice-throwing) and generalizations about the agents and laws involved in the motions of material objects, based on a vast amount of other empirical evidence, direct and indirect, positive and negative.

In connection with the phrase "acquiring information by abnormal means," he distinguishes between "by means which are not normal" and "without any means at all," and asserts that the latter is what the phrase signifies or ought to signify in the mouths of psychical researchers. In this connection Mr. Robinson contrasts telepathy with telegraphy, and says that "if means had been produced," in the former case, "we could all use them." Now there seems to be an ambiguity here in the word "means." One sense of it is a series of events, intermediate in time and perhaps also in space, between one event and another correlated event, so interrelated that we could call the former a "causal *ancestor*" of the latter but not the *immediate* cause of it. This seems to be what Mr. Robinson has in mind when he says that to call a coincidence "telepathic" is "to deny that any *indirect* causal explanation is possible." Another sense of "means" is something that a person can do or something

that he can use in a certain way in order to secure or to facilitate the attainment of some end which he desires. This seems to be what Mr. Robinson has in mind when he talks of a means as "having been produced" in the case of telegraphy, and its not having been produced in that of telepathy.

It is evident that the two senses of "means" are different, and that there is no close connection between them. Suppose, for example, that giving the telepathic agent a certain drug and making certain hypnotic suggestions to the telepathic percipient were found to increase very greatly the rate of scoring in an experiment. Then administering this drug and making this kind of hypnotic suggestion would be a "means" to bringing about telepathy, in the second sense of "means," whether or not the causal relation between an experience in the agent and the correlated experience in the percipient were direct or mediated. Conversely, telepathy might take place through "means," in the sense of a causally intermediate chain of events, and yet we might be utterly without "means" of producing it at will or facilitating it.

The actual position at present may, I think, be fairly summed up in the following three propositions. (1) Most, though by no means all, competent students of psychical research consider that the detailed empirical facts make it almost impossible to hold that, *if* there be causal mediation in the case of telepathy and clairvoyance, it is of any of the kinds *familiar in physics.* (2) There is no consensus of opinion *for or against* the view that these processes are causally mediated *in some other way.* (3) Many of the best experimenters have actively sought and continue to seek for practical means of inducing or facilitating telepathic and clairvoyant powers; but so far little, if anything, has been accomplished in this direction.

It seems to me that much the most important contribution in this Symposium to elucidating the notion of "abnormality" in reference to psi phenomena has been made by Mr. Mundle. He says that what is peculiar and common to such phenomena is that prima facie they involve causal relations between "events separated by a spatio-temporal gap and not mediated by any continuous chain of events," and he suggests, rightly in my opinion, that this is at the root of the discomfort which scientists feel in the presence of such phenomena. He notes another feature which is peculiar to some

psi phenomena, though not common to all of them. This is the temporal clustering of certain events, which must be regarded as effects, about a certain event, which must be regarded as their common causal source, and the fact that some of the effect-events in such clusters *precede* the event which is their causal source.

Now I think that these features are special instances of a more general peculiarity of psi phenomena which may be taken as their distinguishing mark. And I think that it is because of this general peculiarity that psi phenomena should be of certain special interest to philosophers. The point is this. There are certain limiting principles which we unhesitatingly take for granted as the framework within which all our practical activities and our scientific theories are confined. Some of these seem self-evident. Others are so overwhelmingly supported by all the empirical facts which fall within the range of ordinary experience and the scientific elaborations of it (including under this heading orthodox psychology) that it hardly enters our heads to question them. I will call them Basic Limiting Principles. In a recent article in *Philosophy* (Oct., 1949) I stated and discussed eight of these, classified under the four heads of General Principles of Causation, Limitations on the Action of Mind on Matter, Dependence of Mind on Brain, and Limitations on Ways of Acquiring Knowledge. Psychical research is concerned with any alleged event which seems prima facie to conflict with one or more of these basic limiting principles. An event may be very abnormal indeed, for example, the arithmetical feats of calculating boys or the sensory hyperaesthesia displayed by some persons under hypnosis. But, provided that it seems possible to account for it within the framework of the basic limiting principles, even though we may have to postulate hitherto unrecognized laws or agents or both in order to do so, it does not concern psychical research.

Now, if philosophy is what philosophers have always done, these basic limiting principles are of great concern to it, in a sense in which the particular laws and hypotheses of the various sciences are not. And, if psychical research establishes conclusively the existence of facts which conflict with one or more of those principles, it must be highly relevant to philosophy. As I, for my part, accept the antecedents of both these conditional propositions, I hold that psychical research *is* highly relevant to philosophy.

I will conclude my remarks by considering a few points of detail in the contributions to the Symposium.

(1) I am much puzzled by Mr. Robinson's remark that, if telepathy or clairvoyance or precognition were established, this would suffice to refute empiricism. In the first place, he defines "empiricism" in such a way that it would certainly be false quite independently of the results of psychical research. For he says that it is of the essence of empiricism that "we ought not to accept any synthetic proposition except the evidence of our senses." But surely everyone accepts, and rightly accepts, some synthetic propositions on the evidence of personal memory and others on the evidence of introspection. And surely neither memory nor introspection is sensation or sense perception. Secondly, I had always supposed that empiricism was the doctrine that the evidence for all synthetic *universal* propositions is inductive and that the ultimate origin of all our *concepts* is acquaintance with particular instances. Now the information which is alleged to be acquired paranormally is about *singular* facts, not about universal propositions. And it is not alleged that any of our *concepts* are derived from paranormally received data.

(2) I wish to make some comments on certain things which Mr. Robinson says about the possible relevance of psychical research to what he calls the "Aristotelian" and the "Platonic" theories of the nature of the human individual. By the former theory I understand him to mean the doctrine that the cognitive, conative, and affective dispositions and actions and passions of an individual are essentially functions of his ordinary visible and tangible living organism. By the latter I understand him to mean that these dispositions, actions, and passions, or at any rate the more specifically human of them, are functions of something which is existentially independent of a man's present organism, and is only temporarily connected with the latter, though greatly influenced by it during the period of their interconnection.

Now I understand Mr. Robinson to hold that, if the results of psychical research were accepted, the Aristotelian view would have to be rejected in favor of the Platonic. It seems to me that this assertion is plausible only if it refers to those phenomena which seem prima facie to suggest the posthumous existence and activity of

certain human individuals. And even here it is plausible only on the assumption (which is a matter of acute controversy among experts in psychical research) that these phenomena cannot be accounted for by telepathy, clairvoyance, precognition, and unconscious dramatization on the basis of data so obtained, on the part of persons still alive in the flesh. If the Aristotelian view is consistent with all the facts of normal and abnormal (as distinct from paranormal) human psychology, I do not see why it could not be stretched to cover telepathy, clairvoyance, precognition and psychokinesis. Every one of these might have a material basis in certain activities of certain parts of the living human brain and nervous system. On the other hand, even if we were compelled by certain facts established by psychical research to accept some kind of survival of bodily death in the case of some human individuals, it is not at all clear that we should be tied down to the Platonic theory as I understand it. There are all kinds of alternatives, ranging from the full-blown survival of the individual soul animating an organism which is not normally visible or tangible, to the mere temporary persistence of some poor disintegrated fragment of the deceased individual's personality. These alternatives would have to be weighed in the light of a detailed study of the relevant facts. One of the ways in which psychical research is relevant to philosophy is that certain of its data enforce the desirability of considering other alternatives beside the Platonic and the Aristotelian views of the nature of the human individual and suggest alternatives which may be worth exploring.

It is in connection with this question that Mr. Robinson makes the remark that, if the results which psychical researchers claim to have established were accepted, philosophy would be *superseded* by psychical research. Taking this remark in its context, I assume that it must refer, not to philosophy as a whole, but to that part of it which is concerned with the nature of the human individual. Apart from this limitation it seems nonsensical. Even with this limitation the remark seems to me to be a gross exaggeration. The utmost that I could admit is that certain views on this subject, which had previously been *mere* philosophical speculations, would then gain some support from concrete empirical facts. Mr. Robinson's final remark that in any case he "cannot help believing the Aristotelian

view and disbelieving the Platonic view" is a frank confession of invincible prejudice, which is of interest as a detail in his intellectual biography but is not otherwise germane to the subject under discussion.

(3) The last point which I will consider is the suggestion made by Mr. Robinson that philosophers might be usefully occupied in analyzing the terms "telepathy," "clairvoyance," "precognition," and others which are used in psychical research. This is accepted by Mr. Mundle, and I do not suppose that Mrs. Kneale would have any objection to it. I heartily agree, on one condition. That is that any philosopher who undertakes this task should prepare himself by a careful study of the relevant literature, as he would certainly do before venturing to analyze and criticize the terminology of any other special science. For these questions have been and are being vigorously discussed by psychical researchers, some of whom at least have had some philosophical training, and a good deal of analysis and clarification has been accomplished. If this condition is not fulfilled, and if philosophers rush in and pontificate *de haut en bas* on the strength of having hastily skimmed one or two popular books on psychical research they will probably incur and will certainly deserve the accusation of trying to teach their grandmothers to suck eggs.

C. D. Broad

# GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (—1), (—2), (—3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which $n$ is the number of single trials, $p$ the probability of success per trial, and $q$ the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where $\sigma_s$ is the SD of a single run and $R_1$ and $R_2$ are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.